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# Part First.

## ORIGINAL COMMUNICATIONS.

### ARTICLE I.—*Practical Suggestions as to Medical Study.* Being the Introductory Address delivered at the Opening of the Medical School, Edinburgh, for the Session 1869-70. By D. ARGYLL ROBERTSON, M.D., F.R.C.S.E.

GENTLEMEN,—It is not my intention on this occasion, when it has by rotation become my duty to address you from this Chair, to enter upon a philosophic disquisition as to the nature and duties of our profession, or to deliver a learned discourse upon some particular branch of it. It is indeed tempting to one who has the opportunity which has fallen to my lot to-day to enter into matters which, to those of us actually engaged in professional work, possess special interest. But I feel strongly that though this might be interesting to your teachers, and perhaps to a few of the seniors among yourselves, it would not be of practical benefit to those who most need guidance, and for whose advantage I would desire most earnestly to make this lecture available. If, therefore, to those who might be attracted by the more ambitious subject, my remarks should seem too elementary and commonplace, I must ask them to remember, while hearing, as I have endeavoured to do while writing, that this is not so much an occasion for display or self-gratification, as for affording guidance to those who, as junior students, are groping their way through many difficulties. Do I need to remind you of the perplexities which beset you when first you occupied these benches, of the value you would then have attached to a clear indication of the way which you should follow, and of the discouraging effect which an abstruse disquisition would have had upon you in those circumstances? Influenced by such considerations, I have thought it best to direct your attention,—1*st*, to your present position as students of medicine, and the importance of the lines of study you have pursued; 2*d*, to the best methods of mastering the subjects required for the qualification to practise; and, 3*d*, to those subsidiary methods of gaining information, the cultivation of which may entitle you to a higher professional standing. In addition, I may shortly express my views on some points in connexion with medical education, which are engaging the attention of the profession at the present time.

In accordance with recent legislative enactment, it is imperative on all intending medical students to pass an examination in general education preliminary to commencing their professional studies. This examination is now with most of you a thing of the past, and the preparation for it, if thoroughly done, has been, let me assure you, as useful as its result was pleasant. But I think it right to suggest to you the inquiry, which of the subjects embraced in it are worthy of further study, and which of them may with propriety be thrown aside or neglected. English Grammar and Composition, I need scarcely say, merit your life-long attention. All other subjects of general education should lead to this, as their great end and object, that our own language should be thoroughly understood and correctly employed. It is mainly from a want of attention to this, the most essential subject of education, that the medical profession in general has for long failed to occupy that place in literature to which, as a learned profession, she was entitled. It is a reproach, not without foundation, that in medical literature, very few works exist conspicuous for the elegance and clearness of their writing, while our medical journals but too often contain communications which, whatever other merit they may possess, certainly do not exhibit any knowledge of a correct grammatical composition. I remember a gentleman, apparently well educated, and by no mean an unintelligent member of the profession, who announced in a well-known medical journal, that five theories had been assigned as the cause of cholera. Unfortunate theories! they have done much mischief as well as good, but I was not prepared for the assertion that five of them had caused cholera. Inaccuracies—fatal inaccuracies such as this—can only be avoided by improved early education, and by the devotion of any leisure time to the reading and study of the works of standard English authors.

A knowledge of Arithmetic is of such frequent necessity in the ordinary occurrences of life that there is little fear of its essential rules being forgotten. Latin, on the other hand, the only other *essential* subject of examination, is in my opinion not of such importance as to require any further attention at your hands. I think you may fairly place it on the shelf to be taken down when the spirit moves you, and when your energies are not required for any more important object. I am aware that, in expressing this my conviction, I am venturing on a subject on which a great variety of opinion exists. I quite allow that Latin is admirably adapted for the training of the youthful mind, and should form a part of general education; but that it should occupy so large a share of school instruction as until lately has been the invariable custom, is, I maintain, most injudicious. A similar mental training is undergone in the acquisition of German and French, the value of which in after life—at any rate for members of our profession—is infinitely greater than that of Latin. In former days it was wise to insist on a thorough knowledge of Latin, as then prelections on almost all subjects in our Universities, as well as in those of the Continent, were delivered in that tongue, and many, if not most, of the valuable works in medicine were written in Latin; but at the present day such things are almost unknown. The works of ancient authors of merit have been translated, and what was most valuable in them incorporated in more recent works, and comparatively little practical advantage can accrue from the perusal of the old Latin folios. In French, on the contrary, and more especially in German, works of the greatest value exist in the different departments of medical science, the study of which is essential to enable us to keep abreast with advancing knowledge. We need not fear that some slight deficiency in what is called classical attainment will lower us in public estimation; for while, in former days, by a well-educated gentleman was understood one who could occasionally make an apposite Latin quotation, construe a few sentences of Greek, or resolve some geometric problems, but who might be utterly ignorant of all the varied branches of science, and have a mere smattering of philosophy, at the present day that title can only be earned by one who possesses a much wider and more extensive range of knowledge. Modern as well as ancient languages must be familiar to him; physics as well as geometry; science as well as philosophy. The time will come when it will indicate as much ignorance to be unacquainted with the works of Moliere and Goethe, as with those of Virgil and Homer; as much ignorance to be uninstructed in the facts of natural philosophy as in the problems of Euclid.

As illustrative of the insufficiency of a classical education as a preparation for the study of medicine, allow me to quote the experience of one who, towards the end of a University career, after having enjoyed all the advantages of education under men distinguished in classics and mathematics, commenced the study of medicine. He thus forcibly depicts the condition in which he found himself:—

"I knew something of French, but of German, which I soon perceived would be of the greatest use to me, I could not even read the alphabet. For anything I had been taught to the contrary, earth, air, fire, and water, were the four elementary bodies. Why the food I swallowed should not enter my lungs, or the air I breathed into my stomach, I had no information. I found myself completely bewildered by strange words, strange ideas, incomprehensible explanations, lost in a new country where I met no friends, where I could recognise no landmarks. At an age when I ought to have gone through the drudgery of elementary work, I had to commence again with the veriest rudiments."—(Introductory Lecture by Dr Cheadle, at St Mary's Hospital, London, *Lancet*, Oct. 2, 1869.)

I cannot too strongly urge upon you the importance of thoroughly mastering French and German. It is not that you should appreciate the exquisite delicacy and force of Moliere, nor the lofty height and profound depth of Goethe, delightful though these may be, but it is that you may be able without effort to read the thoughts of those brilliant French and laborious German scientific observers, whose achievements only yield to ours in their practical development. The greatest minds have bitterly mourned over the loss of this advantage; and perhaps it were not unfair to say that some men, by no means great in natural power, have been able to earn a high place by its possession. I feel strongly that no circumstance could more retard the progress of a talented aspirant in our profession than ignorance of these languages.

Botany and Zoology are appropriately placed by the Royal Colleges among the subjects for preliminary examination; but instead of being optional, their study should, in my opinion, be rendered imperative. It is really important that a medical man should not display ignorance when questioned as to points of natural history, such as, for example, was exhibited by a distinguished individual, whom I shall not name, some years ago, who, being interrogated as to his knowledge of fishes, announced that he was familiar with them all from the limpet to the whale, omitting to notice that neither of the extremes he indicated belonged to the class of which he spoke. A knowledge of Natural Philosophy, too, is of very great importance in many ways, but especially in enabling us to take a wide and correct view of the circumstances that influence the health of the individual and the community, and of the means whereby an improved sanitary condition may be established. Apart from its own inherent interest, this science is so closely related to Physiology, to Pathology, and to Hygiene, that no medical man of the present day can afford to be ignorant of it. For example, if I may be allowed to select an illustration from my own department, how essential is a knowledge of Optics for a satisfactory acquaintance with the physiology, pathology, and treatment of diseases of the eye. And, on the other hand, do we not observe that the science of optics is every day contributing more to our knowledge of disease of internal organs? Since the ophthalmoscope, by which we can inspect the interior of the eye, was invented, and that is not many years ago, we have got laryngoscopes and rhinoscopes, as well as endoscopes, in which ingenious optical arrangements enable us directly to inspect several deep-seated structures. Or again, to turn to electrical phenomena, how much light have the investigations of Du Bois-Reymond thrown upon the electrical conditions of muscles and nerves, and of Duchenne of Boulogne upon the changes in these relationships in disease, and the benefit which may be derived from the employment of electricity as a therapeutic agent.

We must now, however, turn from the consideration of the preliminary subjects to those which are required to fit you for the important and arduous duties of the medical practitioner. To this end, it is necessary for you to attend a prescribed course of instruction in various branches of medical science.

Instruction in each of the departments is to be obtained in two ways. First, by attendance on a course of systematic lectures, coupled with the careful study of manuals on the subject; and second, by practical investigation and study. The first method, although decried by some at the present day, is, I think, of great importance, as thus alone can the great leading principles of medical science be thoroughly explained and impressed on the students, and because it is the best method of preparing the student to profit fully by the other, the more practical means of instruction.

You might, for example, take a student to a chemical laboratory, and, placing certain solutions in his hands, point out the reagents whereby their nature might be determined; but unless that student had previously studied the laws of chemical affinity and the chemical nature of the substances he employed, the knowledge thus acquired would be of the crudest and most worthless description. Yet this is very much what some extreme theorists would recommend in the study of disease, maintaining that lectures and systematic instruction generally should be curtailed to the greatest possible degree, and all teaching be made practical. On the contrary, I maintain that systematic and practical instruction should go hand in hand, the correct theory or generalization leading to an adequate appreciation of the features of individual cases observed. To confine the student to practical study, would be as great a mistake as could well be made in the present state of science; for just as surely as the investigator must proceed from the special to the general, must the teacher and the learner proceed from the general to the special.

Allow me now to offer a few remarks on the method of study to be pursued. Anatomy and Chemistry are the classes that should first engage your attention, as they constitute the basis on which many other branches rest. In studying anatomy, you should remember that all parts are not of equal importance; that while, for example, a knowledge of the position and relations of the larger arteries of the body is of vital moment, the exact origin and insertion of the multifidus spinæ or spinalis dorsi muscles need not specially engage your attention. Indeed, for my own part, although what I say may be deemed heresy, I do not consider it of the slightest importance to be acquainted with the names of the anastomotic branches whereby the circulation is carried on when one of the larger arteries is ligatured. Such information appears to me to be a simple burdening of the memory. All that should be required is the knowledge that the collateral circulation so established is sufficient to nourish the parts supplied. I do not however mean to defend such ignorance as was displayed by a candidate who once presented himself for examination at one of the Scottish Universities. He had indicated with some correctness the mode of treatment of aneurisms in various situations; but when asked how he would treat aneurism of the arch of the aorta, he suggested ligature of that vessel near its origin. His examiner inquired how the nutrition of the body would be carried on, and was promptly assured that that would be cared for by the collateral circulation through the pulmonary artery. A thorough knowledge of *regional* anatomy is what you should strive to obtain, and that is best learnt scalpel in hand. By assiduous work in the dissecting-room, you further acquire that dexterity of hand which is essential to success in surgery, and exercise those powers of observation which qualify for a correct diagnosis of disease.

The study of Chemistry is so vast, as to render anything like a thorough mastery of it out of the question. Here, I think, you should strive to make yourselves familiar with the composition and commercial source of most of the substances employed as medicines, and become acquainted with the different methods of analysis, especially the volumetric, and that by means of the spectrum. To be ignorant of chemistry often places the practitioner in an awkward position; and it is impossible to be an intelligent physiologist or pathologist, or even a respectable therapeutist, without having earnestly worked at that marvellous science, whose field is the composition of matter.

It has been suggested that chemistry should be placed among the preliminary subjects; but from the intimate connexion that exists between it, and, at any rate, the more advanced departments of medical science, I think chemistry is well entitled to its present position.

Physiology is closely allied to Anatomy. Treating of the appearance and nature of the tissues of the body, and the functions of the different parts of the human frame, it is a most extensive and ever advancing department of science; one in which acuteness of observation and correctness of reasoning are perhaps more required than in any other field of research. Here I would strongly urge you not only to attend lectures on this subject, but also, as far as possible, to study it practically. The minute structure of the various organs and tissues of the body may be described most carefully, and illustrative diagrams and even preparations exhibited; but unless you make the preparations for yourselves—unless, in other words, you, with microscope in hand, examine the tissues for yourselves—the facts are not thoroughly impressed on the memory, and are soon forgotten. We daily read in the newspapers of dreadful accidents, the narration of which may make us shudder, and we picture to ourselves scenes of harassing distress; but still, how soon do other events of varying character absorb our attention and deaden our recollections, so that, perchance, in a month, or it may be a year or two, even the main features of the catastrophe are forgotten. But had we been sufferers in, or even spectators of, the scene, the same events, more vividly portrayed to our minds, would so impress it, that for years to come, perchance to our dying day, they could be readily recalled. Hence the great value of diagrams, experiments before the class, and all of what are called practical modes of instruction, by which information is conveyed to the brain, not by one, but by several of the senses.

In elucidating the functions of many parts of the body, it is necessary to study those derangements of function which diseased conditions of these parts give rise to. This may be done in the medical and surgical wards; but sufficiently characteristic cases, in which disease affects only one distinct organ or tissue without involving neighbouring parts, are of very rare occurrence, and the reasoning on them so open to the fallacy that the symptoms might have been produced by other unobserved diseased conditions, that physiologists have in many instances been obliged to have recourse to experiments on the lower animals, with the view of determining the exact functions of certain tissues of the body. This has been the occasion of much outcry on the part of the sensitive but ignorant public, who, though they see no harm in the enjoyable pastimes of shooting, where but too often the wounded bird or beast escapes for the time, only to suffer the more in the end; or of fishing, in which the poor captive is kept in a state of torture till its physical energies are exhausted, but who yet can inveigh against the cruelty of those who, in the interests of science, and for the welfare of their race, find themselves obliged to subject to pain some of the inferior animals. As long as such investigations are pursued in a scientific spirit, and the operations performed with the avoidance of all unnecessary cruelty, I maintain that the end desired to be attained amply vindicates the means employed. It is, however, I admit, a question how far it is advisable to exhibit such experiments before the class, simply with the view of impressing on the students the results obtained by previous experimenters, and without the object of advancing science by new researches. The only advantage of such a procedure is, that the students are thereby instructed in the method of conducting such experiments, and are thus better fitted to institute researches should the opportunity or desire present itself. This, therefore, is a matter that may be safely left to the lecturer.

One of the most important parts of the Practical Physiology Class is instruction in the use of the microscope. This is not only of service in the study of physiology, but is of still greater practical benefit at the bedside as a means of diagnosis and prognosis, more especially by indicating the nature of tumours, and detecting abnormal conditions of the blood and urine. I think I need say nothing further in recommending its study to your careful attention.

The class of Materia Medica includes two very different subjects, Pharmacy and Therapeutics. The first, which comprehends the nature and composition of the drugs and preparations of the Pharmacopœia, is best studied practically, either at the laboratory of a dispensary or at any large drug establishment where real practical instruction is afforded. But it has often occurred to me that the teachers of this subject should, like those of anatomy and chemistry, themselves superintend the practical teaching of pharmacy in laboratories attached to the schools. A few lessons thus given would be of more value than many hours spent in the ordinary routine work of the dispensary or the shop. Pharmacy might then be less fully entered upon in the lectures on materia medica, and more time be devoted to therapeutics, including dietetics, which is a very wide and difficult subject, sufficient in itself profitably to occupy the whole of the short course allotted to the class.

Having mastered what may be termed the fundamental classes, all your energies should be devoted to the end and object of your studies—the power of recognising and treating disease. Both Surgery and Medicine are taught by systematic lectures, clinical lectures, and practical instruction in the hospital and at a dispensary. All these opportunities of learning should be cultivated to the utmost of your ability; but I need scarcely say that clinical instruction, at the bedside of the patient, is by far the most important. Here you acquire a familiarity with what is hereafter to be your daily occupation. Here alone can you have the means of studying the application of mechanical appliances to the relief or cure of surgical diseases or injuries, of having pointed out to you the characters of various tumours, ulcers, inflammations, etc., whereby their nature is recognised, and whereby a certain method of treatment is indicated. But with this alone you should not rest content. You should, as many as possible, become dressers. The advantages you would thus obtain are very great. The dressing of ulcers and wounds, the bandaging of limbs, the application of splints, the education of the touch, are all matters of daily occurrence, and you get an insight into practical surgery, such as no other instruction can impart. I consider that I am particularly competent to speak on this point, as I neglected this means of instruction while a student, and have often deeply regretted it. Although I regularly attended the wards and sought information, as most of the students did, by attending the visit of one of the surgeons, I completed my studies without ever having seen a fracture set, or having had the opportunity of bandaging a limb. Unfortunately, it is impossible to provide dresserships for all, but during the autumn there is generally no difficulty in getting these posts; and those who fail to obtain them during the sessions, should, rather than neglect so valuable an opportunity of study, be content to sacrifice the autumn vacation for one year. In the medical wards of the Infirmary practical instruction can be more readily afforded to all attending students, and therefore the advantages to be derived from clerkship are not so manifest as in the case of dressership. But still, as clerk, you have to devote more time to the examination of cases; you have more frequent occasion to employ auscultation and percussion; you become familiar with the use of the thermometer, the sphygmograph, and the microscope, as applied to the diagnosis of disease. You thus moreover acquire a practical knowledge of the tests whereby the normal or abnormal condition of the urine can be recognised, and, above all, a familiarity with the method of examining a patient so as most readily to arrive at the important facts of his case. Some students unfortunately deceive themselves with the notion that such instruction can readily be acquired afterwards when they enter into practice, forgetting that they should be as perfect as possible before undertaking, on their own responsibility, the charge of patients, that practice, as a rule, comes very slowly, and that, as years roll on, the senses become somewhat less acute than they were, and much less amenable to instruction. The earlier the senses are trained, the more acute are they capable of being rendered. While attending hospital instruction, be not content with accepting the dictum of the physician or surgeon; look, and, if possible, examine the cases for yourselves, and consider whether you would, unaided, have arrived at similar conclusions. Practise the use of the stethoscope; acquire facility in percussion; seize every opportunity for laryngoscopic or ophthalmoscopic examination, as, after you have entered into general practice, you will find it difficult to instruct yourselves.

The opportunities of studying Midwifery practically in this school are unfortunately not what they should be, and contrast forcibly with the excellent maternity hospitals that exist in connexion with all the Continental Universities. Obvious difficulties exist in giving public practical instruction in this department, but we look forward with hope to the new Infirmary, in connexion with which (though not under the same roof) we trust a model maternity hospital may be erected. At the present your practical experience in midwifery is best to be obtained in conjunction with outdoor dispensary practice.[1]

Pathological or Morbid Anatomy is one of the subjects, a knowledge of which has recently been made imperative by the Royal Colleges. I am strongly of opinion, that attendance on a systematic course of lectures should be made imperative on all students. It is only by a careful consideration of the changes produced by different diseases in various organs that we can hope to arrive at any definite conclusions regarding the treatment and curability of each. It is evident, in the present state of medical science, that to subject any patient to a course of treatment with the view of restoring the function of a part that has been structurally disorganized, would be to excite hopes that could never be realized, and, perhaps, to do positive mischief; while in other cases, knowing from morbid anatomy that the symptoms present arise from and depend upon an inflammatory exudation, our treatment is at once directed to such means as may favour the absorption of the effused matter, and thus cure the disease. Thus alone can scientific medicine stand on a secure basis, and make any real progress. Morbid anatomy, however, which treats of the diseased state of the tissues of the body, should, I think, in all cases be conjoined with general pathology, which includes the causes of disease, a subject which is of itself of absorbing moment in connexion with the increasingly important question of the means of preventing disease. Morbid anatomy can only be studied by attending the post-mortem examinations at the pathological theatre, and by personally examining the morbid structures as they appear to the naked eye and under the microscope. A very good field of study with regard to the general appearance of morbid structures, is the Museum of the Royal College of Surgeons, to which you have all free access. It is particularly rich in excellent surgical preparations, but almost all medical pathological conditions are also represented. It has always been a matter of astonishment that so valuable a collection should so little attract the attention of the students. Any leisure time, however small, may most profitably be spent there.

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[1] I am glad to learn, that this session a course of practical instruction has been instituted at the Royal Maternity Hospital.

The only remaining prescribed class is Medical Jurisprudence, for mastering which a knowledge of all branches of medicine is imperative. This class includes a consideration of all subjects on which the professional opinion of a medical man may be required in a court of law. The majority of the cases in which the general medical practitioner appears in the witness-box, are either cases in which severe injury has been inflicted accidentally, with hostile intent, or suicidally, or cases of sudden or suspicious death. Now, to perform his duty well in such cases, not only is a knowledge of surgery and morbid anatomy necessary, but also a knowledge of the method of procedure to be adopted in performing medico-legal post-mortem examinations. This is more particularly the case with regard to the identification of the body; the precautions with which certain parts should be removed and transmitted to others for further examination, and the forms to be observed in the drawing up of a good medico-legal report. These details, which are of great importance, and upon which very often the life of a suspected person may depend, are most satisfactorily learnt by your actual presence at, and co-operation in, the examination of medico-legal cases—a method of instruction well carried out at this school.

I have now referred shortly to all the subjects, a knowledge of which is imperative; and before proceeding to say a few words respecting certain special branches which are recommended for study, I would like to advert to a few points of practical moment in connexion with your method of working. And first, with regard to the employment of your leisure time during the first year. When you are engaged in dissecting a part, almost the whole of your time will be fully occupied, and any spare hour you may find will be best employed in out-of-door physical exercise. Indeed, I think that, throughout the whole course of study, you should endeavour to keep at any rate one hour in the day apart for the improvement of your health. Thus invigorated, you will, during the first two years, be able to withstand the polluted atmosphere of the dissecting-room; and, during the latter two, ward off infection, or so strengthen your system, as greatly to diminish the inevitable danger. But when, during your first session, you have mastered the bones, and have no part to dissect, you will have considerable leisure time, and that, I think, may be profitably employed in attending the Surgical Hospital. You will, of course, not learn much, but still what you see will, to some extent, prepare you for the classes before you, and give you an interest in the somewhat dry details of anatomy by the foretaste of practical work.

The propriety of taking notes during the lectures is a subject on which some diversity of opinion exists. It cannot be denied that part of the lecture may be lost to the student while he is engaged in noting down what has been said; but the great advantage of notes, I conceive, is that when read, even after the lapse of a short time, they are not only valuable as regards the information they convey, but also, further, to those who wrote them in recalling circumstances, explanations, etc., connected with the facts narrated; while the very fact of writing down what is said, often serves to impress it on the memory. I would therefore recommend you in most of the classes to practise note-taking. Do not take too many classes at a time: the brain is only capable of a certain amount of work; and while by practice that amount may be considerably increased, if care be not taken gradually to regulate the increase, over-fatigue and exhaustion is apt to occur, productive of weakened mental power. It is a well-known law in physiology, that while gentle stimulation conduces to increased power, excessive or long-continued stimulation induces debility. Thus, gentle and intermittent pressure on one part of the skin causes increased growth; but if the pressure be too severe or too constant, it leads to ulcerative absorption. Combine, therefore, moderate relaxation with your study, and you will find your mind the clearer and apter for your work.

Now, while I have referred to all the compulsory subjects of study, there are many special branches of medicine which it would be advisable for all to apply themselves to who aim at proficiency in their profession. I would first specify diseases of women and children. In the class of Midwifery, part of the course is appropriated to the consideration of the diseases of women, but, without clinical instruction, the knowledge thus acquired is most imperfect. I would recommend you all to devote some time to their study in the special wards of the Royal Infirmary. A separate course of lectures is given on diseases of children, and clinical instruction is afforded at the Sick Children's Hospital. When we reflect on the very large mortality that exists during the first few years of life, and the difficulty not only in recognising the nature, but even the existence, of disease, when the poor patient is unable to express in words the nature of his feelings, we at once perceive the necessity that exists of studying the earliest manifestations of disease in children, and of being acquainted with the most approved means of treatment. The fact of the Sick Children's Hospital being a separate institution, and at some distance from the Royal Infirmary, is, I believe, one, if not the chief, reason, why so many students neglect to avail themselves of so excellent a field of instruction. But I would urge upon you the importance of acquainting yourselves with these matters, for children will constitute a very considerable proportion of the patients you will be called upon to treat; and there are few departments where skill is more appreciated and ignorance more readily detected.

Affections of the Eye, although forming a part of the lectures on surgery, are also made the subject of special instruction, both systematically by lectures, and clinically by practical instruction at the Infirmary and Eye Dispensary. It is with some diffidence that I refer to this subject, as it is one in which I am personally interested, and I would have suffered it to pass without special remark, did I not feel constrained to advert to the neglect which this department of surgery suffers at the hands of the mass of the students, and the necessary result of this neglect, that even in the simplest cases of external inflammatory affections, a faulty diagnosis and consequent injudicious treatment is the rule. How often do we observe cases in which faulty treatment is visibly imprinted in the form of a dense white leaden opacity of the cornea! How often do cases of glaucoma come for advice after the curable stage is past, the nature and course of the disease having been misunderstood! How often are patients, whose sight is impaired in consequence of faulty accommodation or refraction, startled by the intelligence that theirs is a hopeless case, that they suffer from amaurosis, and that absolute blindness may be before them! Or similar cases may be subjected to a long course of treatment, the patients confined for lengthened periods to darkened chambers, denied all that can make life agreeable, while their minds are racked with anxious forebodings—all with the view of combating an affection for the cure of which the adaptation of the required spectacles is all that is necessary. Now this ignorance is to be ascribed to the neglect of the means of instruction during student life; for, once engaged in practice, they have neither the time nor the opportunity of acquiring the necessary knowledge. Army and Navy medical men home on leave not unfrequently embrace the opportunity then afforded them of studying several of the special branches, of which in student days they had failed to make themselves masters; but as such opportunities of filling up blanks in practical knowledge are not accorded to most practitioners, I would urge upon you *now* to take the trouble of acquiring skill in these matters, the want of which has been deeply felt by many in the daily duties of practice. This is the period for study, do not neglect it.

What I have stated with regard to the neglect of eye diseases, applies with as much or perhaps greater force to diseases of the Ear. How few students are there who, after passing through the whole of their medical studies, have acquired the least knowledge of these affections. I doubt much if one in a hundred have examined an ear with the speculum, and obtained a view of the membranum tympani. I doubt if as many have ever once employed the otoscope, or even seen it employed. It cannot be denied that diseases of the ear are very intricate, and a large proportion of them incurable; but how can this be otherwise when so little time and attention are devoted to them? The Ear Dispensary is the only place where these affections can be studied. I would strongly recommend you to spend an hour a week there towards the close of your student's career. There are other special studies which are also well worthy of your study, such as Psychology, on which a course of lectures is delivered within the University; Diseases of the Skin and Teeth, which may be studied at the Dispensaries; and Vaccination, a certificate of proficiency in which is now required for the office of Contractor for Vaccination under the English Poor-Law. These are all important subjects, some of which indeed might very properly rank among the prescribed courses of instruction, but time does not permit of my dwelling upon their special claims to your attention.

Scientific investigation and research have of late been very much directed to the discovery of means whereby the occurrence of disease may be warded off. This prevention of disease appears to be best attained by attention to two conditions: 1*st*, the removal of everything that may tend to reduce the vitality of the system, such as excessive or long-continued exertion, ending in exhaustion—insufficient nourishment or ill-selected articles of diet, deficient clothing or cold housing, the breathing of an atmosphere that has not been sufficiently renewed, etc.; and, 2*dly*, the removal of everything absolutely poisonous or pernicious in its effects—for example, the emanations from a sewer, the noxious fumes from chemical or other works, the malarious atmosphere from marshes in certain countries, the poisonous atmosphere engendered by the presence of fever-patients, etc. Much has already been done in this direction, but much still remains to be accomplished; and I doubt not but that, were efficient means taken to isolate completely those labouring under infectious diseases, many, if not all of these deadly plagues, that cut off their thousands annually, might be extirpated.

This subject, which has so much reference to the health of the community, has attracted the attention of the Legislature; and it is now being gravely considered whether a new diploma should not be instituted for proficiency in State Medicine, under which it is proposed to include Legal Medicine (or Medical Jurisprudence) and Preventive Medicine. Now, while I quite admit the great importance of these subjects, I am not prepared to advocate the institution of a new diploma for State Medicine, as I hold that these topics should be studied by all medical men, and should form part of the subjects of examination. In fact, as it is, they are or should be included in the classes of Medical Jurisprudence and General Pathology, and therefore come there under discussion. But as the relations of medicine to the community are so extensive and can be viewed in so many different lights, it might be advisable either to lengthen the course of lectures on Medical Jurisprudence or General Pathology, or to institute a new course of lectures on State Medicine. But there are no grounds for setting it aside from the other subjects of the curriculum, and giving a diploma for proficiency in it. In like manner separate diplomas might be given in Ophthalmology, Dermatology, Psychology, etc. The result of such legislation would be that the general practitioner who did not intend taking this degree would pay no attention to these subjects, and thus the general attainments of the profession would be lowered. It would not be in the power of all those who might not have studied these subjects to keep in all cases free from circumstances in which a knowledge of them would be looked for, and then when perhaps questioned regarding his views in the witness-box a sad and humiliating spectacle might be presented.

A field so wide and rich cannot be explored without hard work, and in working hard, it is of the utmost importance not to work hastily. I cannot but feel that to accomplish all that you have to accomplish in four years, implies hasty work as well as hard; and this surely indicates the desirability of extending the period of study by another year. This has its obvious disadvantages, but what in that way can outweigh practical efficiency? There is nothing more distressing to a conscientious practitioner than to be called upon to treat a case of which, by his own fault, he is conscious he is not the master. Our eminent Professor-emeritus of Clinical Surgery, being thoroughly convinced that the period of professional study is insufficient to enable the student to get a practical acquaintance with all branches of medicine, sees the only solution of the difficulty in one of two methods of procedure,—either to lengthen the period of medical education to eight or ten years, or to limit the extent of information to be required from candidates in each department of their studies, whether for obtaining a license or degree. The first plan he dismisses as impracticable, while the second he considers, though appearing injurious and retrogressive, to be free from objection, and to promise much advantage. Now, while Professor Syme would curtail the number of subjects to be studied, I would prefer to lengthen the period of study, not certainly to eight or ten, but to five years. It is perhaps natural, that to that veteran observer, whose experience has led him to group together much of what he learnt years ago in detail, it should appear possible to educate medical students in a shorter space than we deem needful; but to those who have studied in these modern days, when the facts of science are so multiplied, and the generalizations so little able to keep pace with their increase, it may well appear that, even out of our short lives, an additional year must be accorded to study, if the student is to excel. But how, you may ask, is the additional year to be best employed? I would answer, By the study of special subjects either at home or on the Continent. It is important, perhaps, that I should tell you, in connexion with this, that study and living abroad are not more expensive than here, and that the advantages of such study can scarcely be overstated. For, on the Continent, and especially at the larger German Universities, the teaching of special branches is carried to the highest extent. In fact, almost all subjects are divided and subdivided, and each division or subdivision made the subject of abstruse study. Thus, while in Berlin, I attended five separate courses of instruction in different departments of Ophthalmology. This tendency to subdivision and exhaustive study renders the system of teaching in Germany less adapted to requirements of the junior student than the classes at home; but, on the other hand, it is of infinite value to the advanced student, and more particularly to those desirous of thoroughly mastering one branch of the profession.

The formation of some travelling fellowships in connexion with this school, is, in my opinion, a matter much to be desired. In Russia and in Austria, the State is alive to the advantage to be derived from foreign education, and has instituted a system, whereby the most deserving students are sent at the expense of Government to other countries, the only condition required being that they prepare a report on some special department of medicine, embodying what they observed at the various schools they visited. The Government in this country, however, almost invariably refuses to encourage scientific inquiries, which might lead to the alleviation of much suffering, and the saving of many valuable lives, although it spares no expense in the construction of instruments for the destruction of life.

But the variety of the subjects which are included in professional study, and the rapid strides which medical science is making, renders it altogether impossible for the general practitioner—however thoroughly instructed he may originally have been—to keep abreast in all departments of medicine. New facts are continually coming to light, whereby old theories are overturned, and new substituted. New diseases are discovered, new operations introduced, new instruments invented, new remedies brought to light. Besides, few feel themselves equally adapted for the practice of all branches of the profession—some having a natural talent for surgery, some for medicine, some preferring the more abstruse study of mental alienations. Thus has arisen in medicine what has for long existed in most commercial pursuits—a subdivision of labour. Some are specially or purely physicians—some surgeons—some accoucheurs, etc. At the present day there is a tendency to carry the subdivision still further, and, with advancing science, there is no doubt that specialists will multiply. Against the institution of specialists, it has been urged that general physicians and surgeons, surveying diseases from wider and more philosophic points of view, would treat special affections better than specialists. This objection would have great weight were specialists merely to study one branch of the profession, as was formerly only required for the diploma of the Royal College of Surgeons of London, and is still the case for what is termed the Diploma of Dental Surgery, granted by that College; but this argument falls to the ground when, after studying carefully all branches, one is chosen for special study and practice. Such is the case at the present day—almost all the most learned and celebrated members of the profession being either pure surgeons, or pure physicians, or specialists in some department; and it cannot be denied that it is to specialists that we shall mainly look for medical progress.

I cannot conclude, Gentlemen, without referring to the changes in our staff of Lecturers. To those who recently or at a more distant date belonged to us, and who have been promoted to University Chairs, we heartily wish success, and while we are sorry to see several of our able Lecturers no longer teaching here, we are glad to know that in only one instance have their talents and acquirements been lost to the great Medical School of Edinburgh. On the other hand, we are glad to welcome among us one who has, while officially connected with the University, earned for himself a distinguished position by his original scientific investigations; and not less do we welcome those who, with reputations well earned in their individual departments, propose to supply important wants in our educational system.

I have spoken so much to those who stand on the threshold of medical study, that I cannot but turn now to those of you who have become familiar with what was a few years ago an unknown land. Have you not been struck, let me ask, with the many blanks in our knowledge? Is not the medical field like the chart of Africa or Australia—well defined at some points, in the great majority unexplored? It is never given to one man to make out very much of a country; even the greatest discover comparatively little. That central basin of the Nile, sought for in olden times, and slowly tracked out by Park and Bruce, and Speke and Grant, and Baker and Livingstone, will probably, when more fully made out, only lead to further questions; and these will form the problems to be solved by the heroes of a succeeding age. It is well worth noticing, that some of these individual observers whom I have named were guided in their inquiries by hypothetical considerations; and this affords a further indication of the similarity of these researches to ours. You perceive great blanks in our knowledge. Would you seek to explore them, you must do so by steady work. But how, you will ask, is this work to be accomplished? Learn to know thoroughly the districts already explored. Find out what are the points most important to be discovered. By the light of hypothesis follow out your investigations, and you will almost certainly be led into the discovery of facts which may prove of the utmost importance. Why should we not have more investigations?

Why do we know so little of the exact mode of action of remedies? Why are we so ignorant of the nature of cholera? Why have we so little acquaintance with the morbid conditions which lead to what we term diabetes? What is insanity? What causes epilepsy? What, again, is the nature of fever-poison? What the *materies morbi* of tetanus, of hydrophobia, of chorea (if there be any such)? And, above all, what are the agents by which we are one day to counteract these poisons, and neutralize them as they are generated? I could suggest unnumbered points, many of them of the greatest interest and importance, which you might investigate, and upon which you might found an undying reputation; but let those I have indicated suffice. Any one of them, if properly worked out, would make you remembered while time lasts. Let this be your ambition; but remember, that while all do not achieve such success, there awaits every man the opportunity of quiet, thorough, unostentatious fulfilment of duty, and that these moral things in truth rank higher than the greatest intellectual achievements. So let those who aim at higher things never forget the apparently lesser; and let those who feel themselves constrained to avoid the paths of ambition never forget the dignity of the work they are called to do; and remember, Gentlemen, that in work well done you are not only doing your duty by yourselves, but doing your duty by your neighbour and in the sight of the Supreme Judge who watches over all our careers.

## ARTICLE II.—*Contribution to Vital Statistics.* By JAMES STARK, M.D., F.R.S.E.

(*Read before the British Association at Exeter, August* 21, 1869, *and continued from page* 487.)

### PART II.—*Are any Deaths preventable? If so, are they caused by any particular Class of Diseases? Is there any probability of the Mortality of the Towns being reduced to that of the Rural Districts?*

IN the first part of this paper, it was shown that, from birth to the fifth year of life, the mortality in the towns was more than double that in the rural districts; that from 5 to 20, and from 20 to 60 years, the town mortality was one-half higher than in the country; and that from 60 years till the close of life, one-fifth more deaths occurred in the towns than among an equal number of residenters in the rural districts. These facts therefore proved that, could the mortality of the large towns be reduced to that of the mainland-rural districts, 13,000 lives would annually be saved to the population of Scotland, and the lives of all be prolonged for many years. Even on purely economic grounds, therefore, it is of importance that we should endeavour to ascertain whether there is any probability of the mortality in the towns being able to be reduced to that of the rural districts.

As the basis of such an inquiry, it is necessary that we have undoubted facts, and not mere assumptions; for science cannot move one step towards suggesting the proper remedy, if such is to be found, unless her whole conclusions are based on proved facts. It is the want of such facts which has caused so much money to be expended uselessly on so-called sanitary improvements; and all this has arisen from sanitarians having allowed themselves to be so led away by preconceived theories, that they have only looked at one limited aspect of the question, and so have failed to arrive at the truth.

For instance, in all sanitary treatises, we read a great deal about "preventable deaths;" but as we read on we discover that sanitarians limit that term almost exclusively to "deaths from the zymotic (*i.e.*, epidemic and contagious) class of diseases." They thus assume as an established fact that which has never been proved to be a fact, but which, on the other hand, can be proved to be a most egregious fallacy.

Yet we cannot move a step till we discover what are the diseases or classes of disease which cause the excess of deaths in towns. Is it one disease, or one class of diseases? Or, on the other hand, is it a general increase of deaths from all diseases to which the human frame is subject?

We begin this inquiry by laying it down as an axiom, that all human beings must die;—nay, more, that they must follow the law of nature which is impressed on everything which has life, viz., that they must die at all ages, and that the mortality must be greater the younger and more tender the human being is. Darwin has convincingly shown that this is the case with regard to all the lower animals and vegetables; and the statistics of man prove that in all ages of the world he has been subject to the same law of death. If this, therefore, be the general law of nature, it will be found that the excess of deaths in our towns, instead of being attributable to one or two diseases or classes of disease, will be caused by a proportional increase of the mortality of all diseases. Let us, therefore, ascertain what are the facts regarding this point; for, till that be settled, we cannot move one step towards suggesting a remedy for the excessive mortality in our towns.

Since 1855, a Table has been given in the "Detailed Annual Reports of the Registrar-General of Scotland," showing the proportion of deaths from different diseases in the insular, mainland-rural, and town districts of Scotland. If that Table for any of the years since that date be consulted, it will be seen that every disease which caused death in the towns, cut off its equivalent proportion in the insular and rural districts. This single fact of itself blows to the winds the assumption of the sanitary reformers, that the deaths caused by any disease or class of diseases are "preventable deaths;" because these Tables prove, that though the sanitary reformer could reduce the deaths in the towns by one-third, and thus bring them down to the low proportion of the rural districts, the proportion of deaths from each and every disease, relative to the total deaths, would remain nearly unchanged.

This is a remarkable fact, and one hitherto quite overlooked. Yet, if it shall prove to be the truth—and there can be no doubt about it—it seems to indicate that there is a fixed law of nature which none can escape, which assigns to every one the disease of which he shall die, and that he dies of that disease whether he resides in the town or in the country,—with this marked difference, however, that if his usual residence shall be in the country, he will enjoy a few more years of life; whereas if his usual residence is in he towns, his life will be a few years shortened.

To prove this in a general way is all that even the most perfect system of registration of deaths could do; unless every person who died was visited by a properly qualified medical inspector, who should fill up the certificate of the cause of death. At present it can scarcely be doubted, that through the inattention or ignorance of the informant, or of the medical practitioner who certifies the cause of death, a certain proportion of deaths is registered under a different cause from that which proved fatal. For instance, in the rural districts, the cause of death is often assigned to old age when it was probably induced by bronchitis, influenza, phthisis, or other causes. A certain margin must, therefore, be allowed in all tables of death for such entries; but for all practical purposes, the tables drawn up from the Registers of Deaths may be depended upon—more especially if we limit our comparison to the different diseases in the town and rural districts of the same county.

In the present inquiry, therefore, where we wish strictly to contrast the proportion of deaths from different classes of disease in town and country, the comparison must be limited to the deaths in the town and rural districts of the same county; where it may be assumed that the inhabitants are of the same race, are living on the same kind of soil, are subject to the same climatic and meteorological influences, and that the practitioners who give certificates of the cause of death were educated at the same medical school, and used the same nomenclature of diseases.

Table VI., therefore, gives the results of nine years' registration in the town and rural districts of two of the most populous counties of Scotland, viz., Edinburgh and Lanark. These counties were selected, because they have large town as well as large rural populations; because, being on opposite sides of the island, they are under different climatic and atmospheric influences; and because both town and rural districts are well supplied with a highly intelligent and educated class of medical practitioners, for the most part educated at the University within the respective counties, so that it may be assumed that the town and country practitioners of the same county make use of the same nomenclature of diseases for the registration of the deaths.

TABLE VI.—*Percentage of Deaths from different Diseases to the Total Specified Causes of Deaths in the Town and Rural Districts of the Counties of Edinburgh and of Lanark, during the Nine Years* 1855–1863.

| DISEASES. | EDINBURGHSHIRE. | | LANARKSHIRE. | |
|---|---|---|---|---|
| | Towns. | Rural. | Towns. | Rural. |
| Zymotic Class, | 22·62 | 22·44 | 26·97 | 26·19 |
| Tubercular Class, | 17·85 | 14·04 | 19·13 | 17·85 |
| Brain, etc., | 9·91 | 10·64 | 6·90 | 6·74 |
| Heart, etc., | 5·61 | 5·90 | 3·04 | 2·92 |
| Respiratory Organs, | 14·92 | 13·05 | 17·01 | 13·09 |
| Organs of Digestion, | 6·65 | 7·89 | 7·71 | 8·08 |
| Kidneys and Urinary Organs, | 1·82 | 1·33 | 0·65 | 0·83 |
| Smallpox, | 2·23 | 1·16 | 1·91 | 1·37 |
| Measles, | 2·25 | 1·33 | 2·94 | 1·95 |
| Scarlatina, | 4·62 | 5·07 | 4·51 | 5·37 |
| Diphtheria, | 0·81 | 0·99 | 0·65 | 0·79 |
| Hooping-cough, | 3·97 | 2·41 | 5·52 | 2·88 |
| Croup, | 1·39 | 1·66 | 1·68 | 2·30 |
| Diarrhœa, Dysentery, and Cholera, | 2·51 | 3·65 | 3·99 | 4·33 |
| Typhus, etc., Fevers, | 3·35 | 4·40 | 4·49 | 5·52 |
| Phthisis, | 11·52 | 9·47 | 13·26 | 11·76 |
| Apoplexy, | 2·06 | 2·32 | 1·44 | 1·66 |
| Paralysis, | 2·76 | 3·74 | 1·47 | 1·62 |
| Bronchitis, | 8·76 | 7·73 | 9·59 | 7·43 |
| Pneumonia, | 3·37 | 2·82 | 5·11 | 3·89 |
| Teething, | 1·14 | 1·16 | 2·70 | 1·22 |
| Per cent. of Total Deaths to Population, | 2·453 | 1·882 | 2·907 | 1·980 |

By this Table it appears that, in the county of Edinburgh, the zymotic, or epidemic and contagious, class of diseases caused 22·62 per cent. of the deaths in the town districts, and 22·44 per cent., or nearly identical proportions, in the rural districts. Town and country, therefore, exposed to the same atmospheric agencies and climatic conditions, produced the same proportion of deaths from the epidemic class of diseases—although the annual mortality in the country, as compared with the population, was nearly a third lower than that which occurred in the towns.

The great populous and industrial county of Lanark furnished the same result; for the zymotic class of diseases caused 26·97 per cent. of the deaths during nine years in the town districts, and 26·19 per cent. of the deaths in the rural districts—proportions so close that they may be also considered as identical.

In these two crucial instances, therefore, spread over a period of nine years, we have a notable proof that the epidemic class of diseases cannot be styled "preventable deaths;" because, though year by year, the death-rate of the rural districts was one-third lower than in the town districts, the proportion of deaths from the epidemic class of diseases, as compared with the total mortality, was almost identical in town and country. The deaths from the epidemic diseases are, therefore, not preventable.

Let the full significance of this fact be fully understood. To put it in other language, it means that, of a generation of 100 persons in the towns, and a like number in the rural districts, when all had died, it would be found that absolutely the same number of persons had fallen victims to the epidemic diseases in the town and in the rural districts, viz., 22 in each; but with this marked difference, that the mean age at death of the town generation was only $24\frac{7}{10}$ years, while the mean age of the rural generation was $35\frac{2}{10}$ years; so that each of the town generation had lost 10½ years of life, in consequence of his residence in the town.

Though the residenters in the rural districts, therefore, had on the average enjoyed 10½ years more of life than the residenters in the towns, and this greater longevity was annually manifested by the rural deaths being one-third lower than those of the towns, it had not the effect of saving one single life from the ravages of the epidemic diseases—the relative proportion of the generation cut off by these diseases was the very same as if their residence had been in the towns.

This single fact, then, of itself proves that, although we should succeed through the aids of science in reducing the death-rate of our towns to that of the rural districts, this reduction would be effected not through annihilating any single disease, or class of diseases, nor by changing their fatality, but simply and only by so improving the general health of the residenters in the towns that the mortality from every disease would annually be lower, and thus the mean age at death be increased.

Sanitary reformers, however, at present hold that one disease, viz., fever, is the peculiar disease of towns, and that it is caused by the neglect of sanitary arrangements. They, therefore, boldly assert that all the deaths from fever are "preventable deaths," and invariably speak of them as such. What says our Table?

The counties of Edinburgh and of Lanark give a united testimony that typhus fever (under which was tabulated true typhus, enteric, relapsing, simple continued, and infantile fevers) is relatively more prevalent and fatal in the rural than in the town districts. Thus, in the county of Edinburgh, the deaths from typhus constituted in nine years 3·35 per cent. of the deaths in the town districts, but 4·40 per cent. in the rural districts. In the county of Lanark, typhus during nine years constituted 4·49 per cent. of the deaths in the town districts, but 5·52 per cent. in the rural districts.

The circumstance, therefore, of one-third fewer deaths in proportion to the population occurring annually in the rural districts, did not hinder a higher proportion of the deaths which did occur being caused by fever. From this it is manifest, that though we could reduce the annual number of town deaths by one-third, and thus bring them to the low proportion of the rural districts, we would not diminish in the very slightest the relative proportions who die from fever. The deaths from fever, therefore, are *not* preventable deaths.

All diseases of the stomach and bowels, without exception, even including the so-called epidemic diseases, diarrhœa, dysentery, and cholera, follow the same general law as fever in being relatively more fatal in the rural than in the town districts. Thus, diseases of the organs of digestion in the county of Edinburgh constituted 6·65 per cent. of the deaths in the towns, but 7·89 per cent. in the rural districts. Diarrhœa, dysentery, and cholera in the county of Edinburgh constituted 2·51 per cent. of the deaths in the towns, but 3·65 per cent. of the deaths in the rural districts. In the county of Lanark diseases of the organs of digestion constituted 7·71 per cent. of the deaths in the towns, but 8·08 per cent. of the deaths in the rural districts. Diarrhœa, dysentery, and cholera in the county of Lanark constituted 3·99 per cent. of the deaths in the towns, but 4·33 per cent. of the deaths in the rural districts.

But though sanitary reformers have not detected the fact, there is a most numerous class of diseases which are both actually and relatively more fatal to the inhabitants of the town districts. This great class cuts off one-third of the population of Scotland and of Britain, and has never yet been supposed to be under the influence of sanitary reform, but has been most convincingly proved to be chiefly under the influence of atmospheric agencies, over which man has no control. I allude to the whole diseases which affect the respiratory organs, both those which are usually styled inflammatory or sporadic, and those put in other classes, as phthisis or consumption, hooping-cough, influenza, and croup.

Now, no fact has been better established than this—viz., that the inflammatory affections of the respiratory organs increase with the fall, and diminish with the rise, of temperature. The lower the temperature, the greater is the prevalence and fatality of these affections; and as the temperature again rises, and the weather becomes milder, these diseases abate in frequency and fatality. For instance, in the eight principal towns in Scotland, the deaths from the inflammatory affections of the respiratory organs only numbered 211 in August 1866, when the mean temperature was 55°·6; but they increased monthly with the fall of temperature till they numbered 532 deaths in December, when the mean temperature was 41°·6. During the succeeding month (January), however, the mean temperature fell to 32°·9, and the deaths from these diseases rose to 712. The same fact is noticed every year. For the same reason, these diseases are powerfully influenced by the winds which blow over the country, increasing when the cold northerly or easterly winds blow, and diminishing under the genial mild breezes from the south and west; and in our springs, a town sheltered from these northerly or easterly winds will often wholly escape an increase in its mortality, while a town exposed to these will have its mortality increased as if a virulent epidemic raged. Let us see what our Table says as to the relative mortality from diseases of the respiratory organs in town and country districts.

The two selected counties bear a united testimony that the proportional deaths from the inflammatory affections of the respiratory organs are higher in the town than in the rural districts; and that consumption and hooping-cough, which are also affections of these organs, follow the same law. Thus, in the county of Edinburgh, the inflammatory affections of the respiratory organs constituted 14·92 per cent. of the deaths in the town districts, but only 13·05 per cent. of the deaths in the rural districts. In the county of Lanark, the mortality from these diseases constituted 17·01 per cent. of the deaths in the town districts, but only 13·09 per cent. of the deaths in the rural districts.

In the county of Edinburgh, phthisis or consumption constituted 11·52 per cent. of the deaths in the town districts, but only 9·47 per cent. of the deaths in the rural districts. And hooping-cough constituted 3·97 per cent. of the deaths in the town, but only 2·41 per cent. of the deaths in the rural districts of the same county.

In the county of Lanark, consumption constituted 13·26 per cent. of the deaths in the town districts, but only 11·76 per cent. of the deaths in the rural districts. And hooping-cough caused 5·52 per cent. of the deaths in the town districts, but only 2·88 per cent. of the deaths in the rural districts.

Before venturing to draw any practical conclusions from these facts, it is necessary very shortly to notice, that in this country all diseases, and those styled epidemics fully as much as the inflammatory affections of the respiratory organs, are powerfully influenced by temperature—increasing in frequency and fatality with the fall of temperature, and diminishing with its rise. Thus, taking a ten years' summary, January, February, and March are our coldest months in Scotland; June, July, and August our warmest. When the whole deaths in Scotland during the ten years 1855–64 are abstracted in months—the months equalized, and the correct proportion of deaths taken, allowing an average of 1000 deaths per month, or 12,000 per annum—it appears that the proportion of 1193 deaths occurred during February, 1145 in March, and 1134 in January; while in the temperate month of September a proportion of 843 deaths only occurred, in August 856, and in October 858 deaths.

The so-called zymotic, or epidemic and contagious diseases (with the exception of the bowel complaints, which follow an opposite law), obey the same law as all diseases, in that they increase with the fall of temperature and diminish with its rise. We may take any year as an illustration. Thus, during the year 1862, in the eight principal towns of Scotland, the zymotic diseases caused 331 deaths in August, with its mean temperature 56°·9, but increased with the fall of temperature till, in December, with its mean temperature of 43°, they numbered 692 deaths.

Diarrhœa, dysentery, and cholera obey an opposite law, for they increase with rise of temperature and diminish as the temperature falls. Thus, in the year 1863, the eight principal towns of Scotland had only 27 deaths from these diseases in December, the coldest month, while they had 149 deaths from them in August, the second warmest month of the year.

Now, all the above-stated facts lead to certain determinate conclusions, which ought to form the groundwork on which sanitary operations should be based; but the conclusions are adverse to the popular assumptions of the present day.

*First*, No disease or class of diseases in this country is preventable in the proper sense of that term, nor could be prevented or extinguished by any amount of sanitary appliances. It has been shown that though the general mortality in the rural districts was one-third lower annually than in the town districts of the same county, the very same diseases prevailed and proved fatal in each; and the relative mortality of each disease, as compared with the total deaths, was nearly the same in town and rural district. Even though science, therefore, should succeed in reducing the town deaths by one-third annually, it would not effect this reduction by extinguishing fever, or scarlatina, or measles, or any single disease, nor yet by materially, if at all, altering the proportion of deaths by any one disease as compared with the deaths from all diseases.

*Secondly*, We must give up the idea of any disease or class of diseases in this country being *caused* by foul emanations from drains, cesspools, etc. The general health of the individuals exposed thereto may be deteriorated thereby, so that they are rendered more liable than their neighbours to fall victims to any disease which may be going about at the time; but in no case is the special disease *caused* by the emanation in this country. The malarial fevers of hot countries, being under different climatic conditions, have no bearing on the question under consideration.

For instance, we find that (with the single exception of the bowel complaints) the epidemic and contagious class of diseases, and indeed all diseases, increase under the influence of cold, and diminish in frequency and fatality as the weather improves. They cannot, therefore, be dependent for their origination or increase on the foul emanations from drains, manure-heaps, etc. This is a conclusion totally adverse to the present assumption of the sanitary reformer, who has formed his views, as to what ought to be in this country, from the study of diseases unknown in Britain, and which originate under different climatic conditions. If all diseases, or even the epidemic diseases, were dependent for their origin and prevalence on foul emanations from corrupting matter, they would be most prevalent and fatal at that period of the year when these emanations are given off and are most virulent. But instead of that, these diseases in this country are at their minimum at that period, and only begin to increase as the foul emanations become more feeble; and actually attain their greatest prevalence and fatality at that period of the year when scarcely any or no putrid emanations are given off. As in this country, therefore, the epidemic diseases, as to their periods of greatest prevalence and fatality, follow the same law as diseases of the respiratory organs—which all acknowledge to be solely caused by atmospheric vicissitudes—we are forced to give up the idea of epidemic diseases being *caused* by foul emanations. Though we get rid of all foul emanations, we should still have among us all the diseases which at present cut off our race.

But there is one small class of diseases in this country which increases with the heat, and keeps pace with the virulence of telluric and foul emanations—viz., the bowel complaints; but these diseases, instead of being most prevalent in the towns, are, relatively to the other diseases, most prevalent and fatal in the rural districts. If we look with a scientific eye on that class of diseases, we shall have no difficulty in convincing ourselves that such ought to be the case. All bowel complaints are closely connected with, and influenced by, the nature of the food and drink passed through the stomach. As the heat increases, the streams, springs, and wells over the country get dried up, and the only water which can be procured becomes more or less highly impregnated with decaying animal and vegetable matters. Imperfectly cooked vegetables and unripe fruit are more freely partaken of; while the heat diminishes the digestive power. As all these press harder on the peasant than on the inhabitants of the town, it is only reasonable to expect that bowel complaints should be more prevalent in the rural than in the town districts.

*Thirdly*, The above facts prove that it is the vitiation of the air in the towns which is the chief cause of the increased mortality in them. The large towns, as a general rule, are better drained, have a more perfect system of sewerage, and are better supplied with water, than the smaller towns. Yet a ten years' review of the deaths in towns of different size shows that in every thousand persons 28 die annually in the principal towns, 24 annually in the large towns, 21 annually in the small towns, while in the rural districts where there is no drainage or sewerage at all, and no good supply of water, only 16 deaths annually occur in every thousand persons. Hence it is as clear as facts and reason can make it, that in every case the mortality is in exact proportion to the vitiation of the air which the inhabitants breathe—being in exact proportion to the crowding together of human beings—for the larger the town the more is the air vitiated.

This conclusion is borne out by the fact above adduced, that while one-third of our race in this country dies from affections of the respiratory organs, these diseases are both relatively and actually more prevalent and fatal in the town than in the rural districts. As the air breathed in the towns was more vitiated than that breathed in the country, so the tendency to lung disease was greater. The very air breathed, brought with it the sure seeds of death.

If, then, the vitiation of the air in the town be the principal cause of the increased mortality there, it satisfactorily accounts for the fact observed, that it affects all diseases alike, and that the increased mortality in towns is not confined to one or two epidemic diseases, but extends over every disease. No other cause known to us, excepting atmospheric causes, could equally affect every disease to which town and rural populations are subject. Every disease prevailing with the one, is found among the other; the whole diseases in both localities diminish with the fine weather and increase with the cold. And the sole difference between the two populations is, that the one breathes a purer air than the other; and that the mortality is lower just in proportion to the purity of that air.

Why is it that the mortality increases regularly with the size of the town, as shown by the above ten years' statistics? Because the greater number of human beings collected into one place vitiate the air by the exhalations from their bodies just in proportion to their number. The vitiated air, then, does not cause disease, but simply acts by depressing the vital energy and rendering the body less able to resist the attacks of disease. And the depression of vital energy, and lower tone of the system, will in every case be just in proportion to the size of the town, and consequent vitiation of the air which its inhabitants breathe. This gives a rational explanation of the fact that, though the rural districts have annually a mortality one-third lower than that in the towns, it is the same diseases, and nearly in the same proportions to each other, which prove fatal in both; and that the various classes of towns have each a mortality proportioned to their size.

Knowing, therefore, these facts, and accepting these conclusions, we come to consider our last query: *Is there any probability of the mortality of our towns being reduced to that of the rural districts?*

Looking at the facts, and considering the law of nature which has been pointed out, we would unhesitatingly answer, No! Yet we are convinced that the general health of our towns might be somewhat improved, but not by following out our present faulty sanitary legislation.

All our sanitary legislation in Scotland has failed to improve the health of the people, from mixing together, as if they were one, two things which in themselves are quite distinct, viz., legislation for the general health of the people, and legislation for their greater comfort and convenience. All our sanitary and police Acts make the former to be subsidiary to the latter. Hence, after millions of pounds have been spent on sanitary or police improvements, our annual mortality, instead of having diminished, is rather on the increase. And why is this? Because the first requisite of proper sanitary legislation is to give a better supply of pure air within the dwellings; whereas the operations prescribed, having been chiefly for the convenience and comfort of the masses, have vitiated the air within the dwellings, and made it more impure than before legislation meddled with it.

Before sanitary legislation meddled with our towns, there were very few sinks or other conveniences in the houses inhabited by the great majority of the people. The foul air in the drains or sewers could, therefore, find no entrance into the house, and the impurity of the air within the dwelling was alone caused by respiration, and by the exhalations from the bodies of the inhabitants. But modern legislation enacted that water, and a sink, with soil-pipes, should be introduced into every house, and defined as a house within the meaning of the Act, " every house or part of a house occupied by a separate family." At the census of 1861 it was ascertained that 35·2 per cent. of the families in Scotland lived in houses of one room, 37 per cent. in houses of two rooms, and 11·4 per cent. in houses of three rooms. 83·6 per cent. of the people, therefore, lived in houses so small that this legislation necessitated the introduction of sinks and other conveniences into the very rooms inhabited by the living. From the peculiar construction of the houses in Scotland, this enactment opened up almost every room to the foul exhalations arising from the sewers, through and along the soil-pipes and sinks; and no amount of trapping, and no attention to keeping the sink clean, can prevent the sickening odour of the foul sewer air being perceived over all the house. But later legislation has aggravated the evils still further, by enacting that water-closets shall be erected in such houses; and since these have been introduced, the foul air in the drains has been rendered more deleterious than ever.

In fact, modern legislation has merely transferred into the dwellings themselves the foul air from the open street or lane, where it certainly offended the sense of smell, but did comparatively little harm; and by increasing the virulence of that foul air, and forcing every one to breathe within his dwelling a more vitiated air than formerly, the general health of the people has been deteriorated, and the mortality has been increased.

It is impossible by any amount of trapping to prevent the foul air in the sewer finding its way into the dwelling. It is a law in physics, to which all drainage phenomena are obedient, that if water be thrown down any pipe, well, or shaft, it causes a rush of air in the opposite direction; and it is by taking advantage of this law that wells or shafts may be cleared of foul air by pouring water down them. Every volume of water, therefore, thrown down a soil-pipe displaces at least an equal volume of foul air, which rushes into the apartment through the trap; and no ventilating of the drain, and no amount of trapping, as I have experimentally proved, will prevent this reflux of foul air. But if th ends of the sewage pipes terminate in the sea at low-water mark, from the moment the tide turns and covers the exit, the foul air in the whole drains and soil-pipes connected with the sewage pipes is forced through the traps into the houses, and continues to gurgle through so long as the tide is flowing.

If, in addition to this, the junctions of the soil-pipes with the drains are faulty, or are carelessly made, as they almost always are, every flat of the tenement is inundated with the foul air from the drain; because the soil-pipes are carried in a free space through every flat, and no care is ever taken to build round the pipes as they pass through each floor, so as to prevent the foul air of the lower floors rising to the upper.

But another danger has arisen from this faulty legislation, which threatens, some of these days, to become a most serious evil. In consequence of connecting every floor in these houses with one another by means of these unenclosed soil-pipes, contagious diseases originating with one spread to every other family in the tenement. The free space round the soil-pipes allows the contagious air of the sick apartment to have free access to every family in the tenement. It has even been found that fever and other epidemic diseases may be communicated to families living in different tenements of the same street, through the air within the sewers and soil-pipes carrying the infection, though every ordinary precaution was used to avoid contact with the sick or their attendants.

It could easily be shown how these evils might have been avoided. The drains used for carrying off the moisture of the soil ought never to have been converted into sewers, for if they are to drain the ground they must be made with open joints at intervals to allow the moisture to percolate from the soil into them; whereas, if converted into sewers, instead of draining the ground they saturate it with foul corrupting water. No soil-pipe ought ever to be carried up within the walls of any tenement which is to be let in rooms or flats to families; and the sewers which carry off the soil ought never to pass through the ground floor, but always be external to the walls. If water is to be supplied to each floor of houses, there should be no cisterns or sinks in connexion with the water-tap; but the water-pipe, well built into the wall so that no air could pass along it to the floors above, should terminate in a patent screw tap; and as there is no drop from such a tap, a saucer or plate placed below it would catch any stray drops which might fall. The water of course would require to be kept on constant service, and a cock would require to be put on the water-pipe where it enters each floor to allow the water being turned off during any necessary repairs of the tap. As such an arrangement would prevent all possibility of waste, less than half the quantity of water supplied to any town at the present moment would give a full supply to all the inhabitants. Sinks and water-closets, if thought necessary, should be provided in buildings either separate from the tenement or close to it as a projection, yet outside its walls. This should be freely ventilated, in fact open to the air, so that no foul air escaping into it from or along the soil-pipes could find access into the rooms inhabited by the living. In no case should sinks or other conveniences be allowed within the tenement itself; and old tenements should never be perforated with soil-pipes.

Till something similar to the above be done, and the air within the dwelling be freed from the noxious effluvia which modern legislation throws into it, we can expect in Scotland no amelioration of the public health nor decrease of the mortality.

## ARTICLE III.—*Three Cases of Recovery from Pyæmia, in all of which the Secondary Complications had been distinctly marked; with Remarks on Prognosis and Treatment.* By JOSEPH BELL, F.R.C.S., Assistant-Surgeon, Royal Infirmary.

I HAVE no intention here of even alluding to the question of the exact pathology of the condition called pyæmia, which is so frequently the cause of death after operations in hospitals, and I will take for granted a previous knowledge of the main features of this frightful complication. Indeed, they are too familiar to need much recapitulation.

A patient who has suffered from some severe injury, or undergone some operation—perhaps a very slight one—but generally implying a certain amount of suppuration, is noticed to be hot, restless, and feverish, with an accelerated pulse, and perhaps a dry tongue. Without much warning, he is attacked by a rigor of extreme severity, followed by a profuse and exhausting perspiration. These rigors and sweats follow each other at ever lessening intervals. The colour becomes icteric; the conjunctivæ even become yellow; the face is pinched and anxious; the breath is hot, hurried, and has the peculiar mawkish smell of ill-cured new hay; the mind begins to wander; an exhausting colliquative diarrhœa sets in; and the patient dies exhausted, sooner or later according to the violence of the symptoms and the strength of the constitution.

The extreme and appalling fatality of this frightful disease is well known. I will quote from a few of the best informed and most recent authorities on this subject.

*Danger of Pyæmia.*—I quote from Professor Spence's vivid description:—"Soon the rigors become much more frequent, and are followed by profuse perspiration. The peculiar odour of the breath becomes more marked; the pulse rises very high; the skin assumes a general yellow tinge; pain is felt over the liver; bronchitis supervenes, and vomiting of dark-coloured matter and delirium soon herald a fatal issue. . . . As regards treatment, our chief ai should be prophylaxis, directed to the improvement of the patier general health, before an operation, for we can do very little when pyæmia once sets in. All we can do is to try, by supporting the patient's strength, to let nature have as good a chance as possible of eliminating the poison, and effecting a cure."[1]

Nelaton says the disease is invariably fatal. "Death is the most ordinary result," remarks Velpeau. "In surgical practice," says Bristowe, "nearly all pyæmic cases die. In midwifery practice a larger proportion probably recover." Savory says:—"Of all diseases which the surgeon has to encounter, pyæmia is the most destructive and the worst. When it occurs, recovery is comparatively so rare—and it (the disease) does occur so frequently—it is often so sudden and unexpected in its onset, often so insidious in its progress, and often kills so rapidly, that it is justly regarded by surgeons as the most mischievous of evils. It very often defies all efforts of prevention, and seems hardly ever to be influenced by any attempt to cure. Seldom giving any warning of its approach, it will at once convert a case, which just before seemed full of promise, into one past all hope of recovery; for it cannot be denied that, with rare exceptions, to pronounce a patient the subject of pyæmia, is to say that he is a doomed man."[2]

Professor Gross says:—"The prognosis of pyæmia is most unfavourable; few patients recover, and these, for the most part, remain in a debilitated and crippled condition, liable to other attacks of disease from the slightest causes. Of forty cases, mostly gunshot fractures, treated at the Letterman Hospital, after the battle of Gettysburg, not one, as I am informed by Dr J. M'Arthur, was saved."[3]

M. Chassaignac, at the late discussion on the subject, said:—"In considering the question of the treatment of pyæmia, we are at once met by the fact, that cases of recovery are extremely rare."

M. Gosselin has observed two cases of recovery, M. Broca four; M. Verneuil has seen one. He says, "Out of the myriads of cases of purulent infection, one can hardly reckon up twenty authentic cures. Hydrophobia and tetanus are scarcely more fatal."[4]

Mr Callender says, in his article on Pyæmia, in Holmes's Dictionary:—"Few recoveries from the acute secondary complications are recorded; and even in these the symptoms mark an early stage, when the existence of this disease has been suspected rather than proved. In isolated cases, patients have recovered after severe rigors, followed by some of the local symptoms, as dyspnœa. Expectoration of puriform mucus from the lungs, the passage of bloody and copious stools, observed in these recoveries, have been regarded as eliminatory processes, by which the system is relieved of the morbid material influencing the blood."—Pp. 259, 260.

"What is to be said of the prospect of recovery from the secondary complications? Taking those cases in which no doubt has existed of the nature of the disease, is it probable that a fatal result invariably ensues? It is true that some cases are recorded in which patients have recovered; but in few of these is the existence of pyæmia certain. It is probable that a fatal result must generally be anticipated; for all practical surgeons acknowledge that very little chance remains for the patient who, after an operation, is attacked with the symptoms of the disease."—Pp. 279, 280.

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[1] Lectures on Surgery, part i. pp. 33, 34.
[2] Braidwood on Pyæmia, pp. 256, 257.
[3] System of Surgery, vol. i. p. 150.
[4] Gazette Médicale de Paris, No. 25, 1869.

A case lately brought under the notice of the Academy of Medicine of Paris by M. Alphonse Guerin elicited an interesting and valuable discussion. It was one illustrating the curability of pyæmia. A young man, after a bruise of the thumb, had an attack of pyæmia, in the course of which he had violent rigors, pain in every limb and in his liver, great prostration, with profuse sweats, dryness of the skin, metastatic abscesses in his legs, etc. He was treated by large doses of quinine, 30 to 45 grains per diem, and recovered. Five months later he hanged himself. His body was opened, and on the surface of the liver numerous umbilicated cicatrices were found, doubtless the result of metastatic abscesses which had formed in that organ.

In the discussion which followed the reading this case, many doubts were raised as to the value of the remedy, in answer to which M. Guerin stated that he was aware of other facts, which tended to prove the value of the remedy. His experience was, that if, in a patient suffering from pyæmia, the quinine produced deafness and buzzing in the ears, his recovery was hopeless; but if there was such a tolerance of the remedy as to bear well doses of from 30 to 60 grains per diem, the prognosis became more favourable.

M. Broca detailed four cases, which I can here only notice.

1. An author, after an operation on his anus, had pyæmia, evinced by pulmonary abscesses and an enormous abdominal abscess, and recovered.

2. A mechanic, after an injection of perchloride of iron into a varicose vein, had phlebitis and the symptoms of pyæmia—abscesses all over the body—one special one in one shoulder-joint. After hovering between life and death for fifteen days, he recovered.

3. The third recovered partially, but died of the results of the pyæmia in eight months. He had fifteen metastatic abscesses, one in the left knee, another under the scapula, another in the thigh. The fourth was still under treatment.

His view of treatment was that it was a course of hygiene, not any special drug, that cured (*une médication, et non un médicament*). It was a strife between the organism and a poison, which was not lasting, but temporary; if the organism could be strengthened to resist for a time, it might gain the victory. He prescribed alcoholics and a tonic regimen.

Chassaignac believes that now and then a patient may recover, let the treatment be what it may.

Dr Braidwood, from whose excellent work on this subject I have obtained references to some of the above prognoses, gives very wisely an opinion more encouraging; yet, of the nineteen cases observed by him with such care, all died.

I have been led to record the three following cases in the hope that the experience of those of my colleagues who have seen more of this dreadful disease than I have, may be elicited as regards treatment, and because, in some slight respects, the treatment I adopted varied from that recommended in the surgical text-books. I know how little value can be given to these isolated cases—and know that two of them, being in adolescents, had a greater natural tendency to recovery than if they had been older; still, in all, the symptoms were most unequivocal and unmistakable, and thus they form no mean addition to the meagre list of recoveries from acute pyæmia.

Chronic pyæmia, about which Mr Paget has not long ago written so well and clearly, is a quite different disease, and has much more chance of a favourable issue.

CASE I.—Donald Morrison, æt. 23, was admitted into the Infirmary, 19th December 1868. For the last three years the patient has suffered pain, swelling, and hardness of the lower two-fifths of the right tibia. The bone is irregularly thickened, chiefly near the ankle. The skin of the leg is glazed and brawny, and there is an ulcer about the size of half-a-crown on the front of the tibia. Patient states, that at times he has the most intense gnawing pain in the limb, which is relieved only by the escape of matter.

*7th to 14th January.*—During this week the patient has suffered intensely from this pain, being unable to sleep. His health is also suffering.

*15th.*—To-day Mr Syme trephined the tibia, and, removing tolerably thick crown of bone, gave vent to a small quantity of curdy pus.

Dr Lawrie, the house-surgeon, used the carbolic antiseptic precautions with very great care, syringing out the abscess and the cavity of the bone under cover, and swathing the whole limb in lint, soaked in carbolic lotion. *18th.*—Discharge very profuse. *20th.*—Excessive discharge; whole limb up to knee glazed and swollen; intense pain; delirium; pulse 120. *23d.*—An abscess has formed, opening into the ankle-joint. The delirium is so violent that patient had to be removed to Ward X. Pulse 148; tongue furred.

From this date to 29th January, symptoms were unabated. A most profuse discharge weakened the patient. His tongue was dry. He was wildly delirious.

*29th January.*—The patient came under my immediate charge. On examining the limb, I found the tibia quite dead to the knee. A most profuse discharge came from the whole medullary cavity and from the open ankle-joint; but though excessively weak, he is not quite so delirious, and the tongue is beginning to clean; but, on the other hand, he to-night took a most violent rigor—the first.

*30th.*—I removed him to Ward V., as he was not so noisy. Another rigor; the pyæmic odour began to manifest itself.

*31st.*—To-day, as his only chance, I amputated, by two long rectangular flaps, through the lower third of the thigh, sawing the condyles off with a slightly rounded curve. Four vessels were tied by the prepared catgut ligatures, the rest were twisted; the stump was dressed with the carbolic acid paste.

*Vesp.*—The flaps had to be taken down on account of hæmorrhage from the vascular periosteum.

*1st February.*—Fair night; pulse 140, very weak.

*2d.*—Pulse 138; considerable bloody discharge. Ordered port, ℥viij. *Vesp.*—Pulse 148; to have ♏15 of tr. ferri perchloride, and ♏40 sol. mur. morph.

*3d.*—Discharge purulent, with putrefactive smell. Dressing removed, and flaps syringed with carbolic lotion, 1-60; to have ℥iv. of brandy, in addition to the wine.

*6th.*—Improving; an abscess on outer side of thigh required evacuation.

*12th.*—Still improving, notwithstanding profuse discharge.

*14th.*—Another abscess forming high up thigh.

*15th.*—This was opened close to trochanter, under carbolic acid.

*17th.*—Pain in chest and cough weary him much; ordered a simple cough mixture, and constant poulticing to chest.

*19th.*—Spat up a quantity of pus, coloured with blood. Extensive posterior dulness of left lung.

*22d.*—Another secondary abscess formed in front of thigh, and required evacuation.

*25th.*—Condition of patient is now much improved in every way; stump is tolerably well consolidated, though sinuses extend up to the nearer abscesses. It is dressed with carbolic plaster to keep down smell.

*5th March.*—Since last report, another secondary abscess has formed, and required evacuation. He is now gaining strength.

*20th.*—Patient has sat up for the first time. The stump is now quite healed, with the exception of a small granulating sore, like a sixpence.

*21st May.*—Patient dismissed cured, having gained enormously in weight, and now being hardly recognisable.

CASE II.—William Morris, æt. 17.—Patient, rather a delicate-looking and nervous boy, was on a ladder, and fell from it, a distance of about 8 feet, on 26th June 1869, fracturing his left tibia and fibula.

On admission to hospital, shortly after the accident, it was found that a compound fracture of tibia existed an inch or two above the ankle. There was considerable hæmorrhage, the anterior tibial artery being wounded. The fracture was set and dressed after Mr Lister's carbolic acid plan. The fibula was broken at usual place. For three days the case seemed to progress favourably, notwithstanding considerable restlessness; but on 30th June his pulse rose, his tongue became dry, and he was delirious at night. The foot was still quite warm, and slightly red.

31*st.*—To-day one or two ecchymotic bullæ were seen on the ankle. He was wildly delirious during the night, throwing the limb about; and at 2 A.M. the toes became quite cold and blue.

1*st July.*—To-day, at visit, it was obvious that moist gangrene had involved the whole of the leg, and already extended above the knee. His parents were sent for, and at 12 o'clock I endeavoured to obtain leave to give him the last resource of amputation. He was already very weak; pulse 130, thready, compressible, and he was in a state of low, muttering delirium.

The parents absolutely refused permission, and tried to remove him from the hospital; this, however, the boy resisted, and at 4 P.M. I was sent for by them, and now they requested me to amputate.

By this time the gangrene had spread higher, and I had to amputate at the junction of the upper and middle third of the thigh, indeed just below the trochanters. I did so by a long anterior and short posterior flap, secured the vessels by catgut ligatures, and dressed the stump after Mr Lister's method. The wound was sewed up with silver wire, and the stitches all tightened, except two in the most dependent part of the flap. Here there was inserted a carbolized plug of lint.

2*d.*—Patient much better; pulse 108 in morning; 96 at visit. Under a stream of carbolic acid lotion, the wound was now dressed, all serum and blood squeezed carefully out, and stitches which had been left loose were tightened. The whole was carefully surrounded by two layers of the carbolic acid and shell-lac plaster, over which was placed a cap of oil-silk and a bandage.

3*d.*—Flaps at edge are looking dark and doubtful; patient restless and uneasy.

10*th.*—After a week of restlessness and suffering, the whole of the anterior skin flap has now sloughed away *en masse*, leaving a large granulating surface, with profuse suppuration, notwithstanding the very careful and regular use of the carbolic acid dressings; during this week the patient's pulse has averaged 120, and he has been feeble, and occasionally delirious.

11*th.*—He had a very distinct and severe rigor.

12*th.*—Another one; each followed by very severe and exhausting sweats. He is put on quinine in three-grain doses thrice daily.

14*th.*—Another rigor, but not so severe or long-continued.

17*th.*—Very great care has been taken to check rigors by use of hot drinks, whenever they seemed about to commence. There has been no well-marked ones since 14th, but numerous slight chills, and copious exhausting sweats. Quinine has to be stopped, as it sickens him.

*25th.*—Since last report, patient has been gradually improving in health and strength, though he is still very feeble. He has a distinctly localized pain in left chest, and percussion and auscultation easily map out a limited dull space, evidently containing fluid, and probably a metastatic abscess.

*1st August.*—To-day patient confirmed the diagnosis by spitting up, in a few coughs, about a teacupful of fœtid pus, to the great relief of all his symptoms.

From this date, he steadily and rapidly improved; the wound healed rapidly; the bone, which for a time projected, became covered with granulations without any exfoliation taking place; and on 4th September, when he was dismissed cured, the sore had contracted to the size of a shilling.

Case III.—Matthew Smith, æt. 14, while playing near a stationary engine of which he was in temporary charge, got his right leg caught in it, and smashed to the knee, 5th June 1867. On admission, I amputated above the knee-joint, by Mr Carden's method. Nothing of note occurred during the operation, or for the first three days after it, during which the boy was remarkably well.

*8th June.*—The edge of the flap began to look unhealthy.

*15th.*—Very rapid hospital gangrene has destroyed the greater part of the long anterior flap, and caused protrusion of the end of the bone.

*1st July.*—For the last fortnight, the face of the stump has made little progress. Discharge scanty, tending to recurrence of gangrene at edges, notwithstanding the free use of fuming nitric acid, etc. To-day he had a very severe and continued rigor.

*7th.*—Up to this date he has had numerous rigors, always increasing in frequency and severity. Opposite ankle and calf swollen. Exhausting diarrhœa; still the pulse is not above 108; and he is able to take milk with lime-water, and beef-tea, which he gets most regularly.

*8th.*—To-day the rigor was less severe.

*10th.*—Another rigor.

I need not give daily reports. From the 10th, no more distinct rigors occurred; appetite improved still more, and pulse came down to an average of 96, yet, within next six weeks, we were able to trace three pyæmic abscesses at least. One in left lung, easily diagnosed by dulness and auscultatory phenomena, and verified by the expectoration at once of more than half a pint of fœtid pus. One in liver, diagnosed by severe localized pain, requiring numerous poultices, and verified by the passing by stool of about a pint of pus. Another in left kidney, evinced by the pain and swelling, and verified by the passing of a very considerable quantity of pus in the urine.

By the end of the six weeks, he was very weak in body, and mentally almost fatuous: from being a bright, active, cheerful lad, he had changed into a mere eating machine, who had no words; could not tell the doctor's name, indeed could hardly be roused. In two successive days, he had forty-five epileptic fits of great severity, after which, for some hours, he was in a comatose condition, and supposed to be dying. However, his stomach never failed him. His beef-tea and milk were taken greedily, and digested, and towards the end of September he began to rally. His stump rapidly healed, and on 7th October he was able to be taken home, with stump consolidated, except over a large ring of necrosed bone not yet loose. He was ordered to return to be re-amputated, as I feared his stump would be useless. However, in about six months, he came back with the exfoliation in his hand; and he has now a very good, firm stump, and is a fine lad, healthy both mentally and bodily.

I have said very little in the notes of the daily progress of my three cases regarding the treatment adopted, the medicines given, or even the stimulants allowed.

In the charge of all the cases, I directed my attention not to any hope of finding antidotes, or chemically acting on the poison. I gave no alteratives, no specifics, no mercury, but simply aimed at, *1st*, keeping the primæ viæ in such a state as best might aid assimilation of nourishment; and, *2d*, giving the nourishment in such a form as most easily might be taken up.

Milk, with lime-water, eggs, beef-tea, at short intervals, and in small quantity, were the chief remedies; an occasional purgative enema, or even a purge of castor-oil when required, and really very little else. Case I., from his excessive weakness, had for some days ℥viii. of port, and ℥iv. of brandy; but this was soon diminished. Case II. got quinine, but could not bear it; at least his appetite diminished under its use, and I had to stop it. Case III. had hardly any drug whatever during his whole long illness. I have more than once (in other cases, never in these ones) tried the internal use of the sulphites, as recommended by Dr Polli, but cannot say that I have ever seen any benefit result. Indeed, their effect on the patient's appetite was generally so bad, as very speedily to cause them to be given up.

Hot drinks, not necessarily containing alcoholics, even hot water, or hot milk-and-water, were found very useful in preventing rigors, which a cold drink would inevitably have hastened. I have been led to lay great stress on the importance of delaying the dressing of the patient's wounds, when his skin is moist, or he feels a rigor to be imminent, and selecting such times as he himself will soon learn to recognise as the most suitable, as involving the least risk of chill.

I must conclude by adding, that no case will ever recover from pyæmia, without the most careful and devoted nursing. I was fortunate in having this assistance in all these three cases. Mr Syme's old nurse, Mrs Porter, aided in two cases by the mothers of the young men, having done her utmost in their behalf.

## ARTICLE IV.—*On Polypus of the Rectum.* By A. G. MILLER, M.D., F.R.C.S.E., etc.

(*Read before the Medico-Chirurgical Society of Edinburgh, 3d November* 1869.)

IN November 1868 I was requested to see Mrs D., who was said to be suffering from an attack of "piles." She had the ordinary symptoms of hæmorrhoids, with this peculiarity, however, that, though a tumour protruded when she went to stool, and required to be pushed up again, she seldom suffered from loss of blood.

On examination there was very little hæmorrhoidal appearance externally; but, on passing the finger into the rectum, two small internal hæmorrhoids were discovered; and at the same time a hard substance was perceived, which, on being touched, floated upwards. This body was easily made to protrude through the anus; and, from the facility with which it could be moved about, was evidently attached by a long pedicle to the wall of the rectum. Believing this to be a polypus, I determined to remove it, and, at the same time, ligature the hæmorrhoids. Accordingly, the former being drawn well down by means of a volsella, I fastened a ligature firmly round the pedicle close to the mucous membrane of the bowel, and then cut off the tumour at a little distance from the ligature. There was no hæmorrhage, and the operation seemed to give the patient no pain. The hæmorrhoids were then tied in the ordinary manner. Chloroform was not administered, as I thought it better to have the assistance of the expulsive power of the patient during the operation, and she herself preferred being without it.

The patient made a good recovery,—suffering evidently from the ligatured hæmorrhoids rather than from the more internal strangulation.

After removal, the tumour was observed to be about the size and shape of an almond, nearly one inch long, and half an inch broad, hard, almost cartilaginous to the feeling, pale, and covered with mucous membrane. A small vessel was distinctly seen entering the pedicle. (In spirit it has lost nearly one-third of its original size.)

The microscopical examination showed the structure to be fibrous.

Polypus of the rectum, of which disease the case I have just read is an example, is described by most writers on surgery as a rare disease. Sir Astley Cooper speaks of having seen ten cases—the greatest number recorded by any single observer.

Various instances of this affection have been published in the French medical journals since 1761,[1] but in 1816, for the first time, as far as I am able to discover, Delpech[2] described polypus as a distinct affection of the rectum. Sir Astley Cooper, in 1823,[3] records a case of what he calls a vascular polypus, and he says that he "never before heard of the disease." In 1825, Boyer[4] speaks of it as existing in two forms, the simple mucous and the malignant. In 1839 Lizars[1] describes three varieties, the *mucous*, *fibrous*, and *fungous* or malignant. Bushe[2] says that the fibrous was described by Sanson in 1833; but as I have been unable to procure the work referred to, I can only state it upon his authority.[3] Syme,[4] in 1845, described a variety of polypus which presented some of the features of villous cancer. Various other authors, for example the writers of our text-books on surgery, describe this disease under the general term of polypus of the rectum, doing very little more than naming the affection. But the most extended writing on the subject is to be found in Holmes's System of Surgery,[5] where Henry Smith describes *five* varieties. He mentions first the folds or "processes of the mucous membrane standing out prominent in the cavity of the rectum," as a variety. The second he calls "soft gelatinous," and describes it as of a "fibro-cellular structure." The third is, like the first, mentioned for the first time by himself under the title of "warty," or epithelial. He places fourth the vascular growth, as observed by Sir Astley Cooper. And, lastly, he deals with the so-called cancerous polypus, which, however, he is inclined to consider as more properly a variety of villous growth. But notwithstanding the number of forms which he describes, Mr Smith omits the fibrous, described by Lizars and Syme, and a specimen of which I have shown you.

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[1] Journal de Médecine, tome xv., Leautand.

[2] Des Maladies Chirurgicales, tome iii.

[3] Lectures on Surgery, *Lancet*, 1823-24. [4] Des Maladies Chirurgicales, t. x

Considering the uncertainty in the arrangement of the different forms of this disease, and the diversity of opinion of the principal authors, I shall offer another, and, I trust, more accurate classification. The simple folds or processes of mucous membrane, described by Mr Smith, I shall not accept as a variety, inasmuch as they are not strictly morbid growths, therefore not true polypi, and even, according to his own account, "cause very little inconvenience, and therefore do not demand the attention of the surgeon."

1. The simplest form of polypus of the rectum is the mucous or glandular, so called according as you consider its appearance or microscopical structure. This variety includes the mucous, vascular, and glandular polypi of various authors, and occurs most frequently in the young. It is spheroidal or cylindrical in shape like a worm, sometimes arising by two roots, or having a bifurcated extremity, and is frequently of a bright-red colour. It is soft, compressible, and frequently covered with a slimy, mucous discharge. The pedicle may be long, so as to permit of the tumour being extruded, or the growth may exhibit its presence by the irritation it produces and the hæmorrhages which come from it. The blood may be discharged from an abraded or ulcerated surface, or may apparently be made to flow simply from the pressure to which the polypus is subjected by the patient straining at stool.

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[1] Practical Surgery, part ii.

[2] On Rectum and Anus, 1837.

[3] Since reading the above, I have procured a copy of Sanson's work, and find that he does mention a fibrous variety. Nouv. Elémens de Pathologie, tome ii. p. 527.

[4] Surgical Pathology, etc.

[5] Vol. iv.

Cases have been recorded where the tumour was detached by such straining.

Paget,[1] Billroth,[2] and Forester[3] give the structure of this variety as being essentially tubular glands, simple, or branching, lined with cylindriform epithelium, such as are found in the mucous membrane of the bowel from whence the growth springs.

There seems to be no part of the rectum more prone to the occurrence of this disease than another. There may be one or more polypi present, but the disease being benign, once removed, it is not liable to return.

2. The second form is the fibrous, also a simple growth.

As described by Billroth, Syme, Quain,[4] and Curling,[5] and observed by myself, this tumour is globular or oval, dense, hard, and insensitive, with a long pedicle, and is usually found in adults. It seldom grows to any great size, the largest recorded having been the size of a walnut.

Billroth describes one as being composed of white elastic fibres. The tumour which I show you presents, under the microscope, the characteristics of fibrous growths as they are found in other parts of the body. The fibres are arranged in wavy bundles, or in the form of a network. Connective tissue is also apparent, and the pedicle consists of that structure with the nutrient vessels embedded in it. The polypus is covered with mucous membrane, which presents no ulcerated or abraded surface.

Sanson is said to have described fibrous polypi as more common than mucous. I have been unable to procure his writings, but from the references to the cases I am inclined to think that he mistook malignant growths for fibrous, as he speaks of them being of a great size.[6]

From the cases which I have been able to collect, I should consider this the rarest form of the disease.

3. In the third and last variety, I include all the malignant forms of polypus which are liable to occur—the epithelial, described by Mr Smith, the scirrhus, by Bushe, and also the villous tumour of Paget, Syme, and Holmes.[7] The characteristics of the malignant polypus are those of cancerous tumours in general; but, contrasted with the simpler growths liable to be found in the rectum, they would seem to have usually a larger base of attachment, a lobulated, sometimes fungating appearance, and a proneness to bleed. When protruded from the bowel the tumour may present the appearance of a bunch of red currants.

I have included in this class the villous tumour, though Mr Paget thinks that such growths (more especially when found in the rectum) may be non-malignant. Mr Holmes has recorded a case where the microscope showed no cancer-cells, but the general symptoms of the disease, its tendency to return and grow rapidly, and the principles which should guide us in treatment and prognosis, are the same as in ordinary cancer.[1]

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[1] Surgical Pathology, p. 451 (2d edition). [2] Bau der Schleimpolypen.
[3] Atlas Mikroskop. Anatomie Sup. taf. xxv. [4] Diseases of Rectum, p. 269.
[5] Diseases of Rectum, p. 56. [6] See Note, p. 614. *Loc. cit.*, pp. 498, 527
[7] Patt. Soc. Trans., vol. ix. p. 212.

The following thirty-nine cases have been selected, as the most accurately recorded, from the works of seventeen authors, published since the year 1761. From these we may, with an approach to certainty, gather the following deductions:—

*1st.* That polypus of the rectum is a rare disease.

*2d.* That the glandular form is most frequently met with, the malignant next, and the fibrous most rarely. (Their respective frequency of occurrence being—24, 8, and 6, in 39; or 61·5, 20·5, and 15·4 per cent.)

*3d.* That the glandular occurs most frequently in children, and the fibrous and malignant in the adults, the proportions being respectively 16 and 6 in the former instance, and 13 and 1 in the latter.

*4th.* That, taking every variety into account, polypus of the rectum is met with as frequently in adults as in children, eighteen instances of each having been recorded.

| Date of Work from which the Cases have been taken. | Author. | No. of Cases. | Variety. | Sex. | Age. |
|---|---|---|---|---|---|
| 1761 | Leautand . . | 1 | Large | Male . | 15 |
| | Trioen . . . | 1 | Scirrhus large | Female | 36 (recur$^{d}$, death) |
| | Le Drau . . | 1 | Malignant large | Male . | 60 |
| 1825 | Boyer . . . | 1 | Malignant large | Male . | 15 (recur$^{d}$, death) |
| 1835 | Cooper . . . | 8 | Glandular . . | | Children |
| " | " | 2 | Glandular . . | | Adults |
| 1837 | Bushe . . . | 1 | Glandular . . | Male . | Adult |
| 1845 | Syme . . . | 1 | Glandular . . | Male . | 18 |
| " | " | 2 | Fibrous . . . | M. & F. | Adult and old |
| " | " | 2 | Malignant vil. . | | Adults |
| 1853 | Schuh(Billroth) | 1 | Fibrous . . . | Female | Adult |
| 1854 | Quain . . . | 2 | Glandular small | | 8 and 10 |
| " | " | 1 | Fibrous . . . | Male . | Adult |
| 1855 | Curling . . . | 1 | Glandular . . | Male . | 5 |
| " | " | 1 | Glandular . . | Female | Little girl |
| " | " | 1 | Fibrous . . . | | Adult |
| 1855 | Förster . . . | 1 | Glandular . . | Female | 18 |
| 1855 | Billroth . . | 1 | Glandular . . | | 2½ |
| 1858 | Holmes . . . | 1 | Glandular . . | Male . | 3 |
| 1860 | Stoltz . . . | 2 | Glandular . . | M. & F. | 8 and 4 |
| 1861 | Holmes . . . | 2 | Villous (malig.) | M. & F. | Old |
| 1863 | Paget . . . | 1 | Glandular . . | | |
| 1864 | Smith . . . | 1 | Epithelial . . | Male . | Adult |
| " | " | 2 | Glandular . . | M. & F. | Adult |
| 1869 | Miller . . . | 1 | Fibrous . . . | Female | Adult |
| | 17 | 39 | G. 24, F. 6, M. 6, (Vl. 2) . . | M. 12, F. 9. | Children 18, Adults 18. |

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[1] The case which Mr Holmes records was operated on thirty-three times during four years, and of Sir Benjamin Brodie's case, which he quotes, it is not stated whether the disease did or did not return.—*Loc. cit.*

And now a few words with regard to the method of removing these growths. The knife, scissors, ligature, and nitric acid have all been recommended and used. But I have no hesitation in saying, after the careful observation of recorded cases and facts, that the ligature is the safest and proper instrument to employ. The polypus may be cut off immediately afterwards, but the ligature should always be applied as a precautionary measure. Several surgeons have cut away glandular polypi, and they have been torn off by efforts of straining at stool, without any serious hæmorrhage occurring. But, from the results of careful examination of these growths after removal, it is evident that their vascularity is such as to necessitate precaution. However nonvascular in appearance, a polypus should not be removed by the single stroke of a knife or pair of scissors. No tumour could have appeared more decidedly nonvascular than the one which I removed, hard, pale, and without sensibility; and yet there was a vessel in the pedicle which, if it had not been secured by ligature, would certainly have given rise to troublesome hæmorrhage. It may be borne in mind also, that the polypus once removed, its origin will not easily be reached and exposed for the purpose of applying hæmostatics. Serious hæmorrhage has occurred after removal of these growths, and plugging of the rectum has been had recourse to on more than one occasion, and therefore we are justified, I think, in maintaining with Syme, Miller, Holmes, and others, that the ligature, either alone or followed by the scissors, is the proper method of treatment.

There have been recorded at different times and by various authors no less than seven varieties of this rare affection, viz., the mucous, the glandular, the vascular, the fibrous, the villous, the epithelial, and the scirrhous, besides the folds of the mucous membrane described by Mr Smith. I have attempted to reduce these to *three* forms, which shall include all the varieties of this disease. If I have succeeded, I shall consider that I have attained my object in writing this paper.

## ARTICLE V.—*Case of Removal of a Sequestrum from the Centre of the Frontal Bone, after twenty-eight years' Suffering.* By PETER BROTHERSTON, F.R.C.S.E., Alloa.

IN the month of May last, E. H. of Muckart consulted me about a sinus, situated a little to the right of the centre of the forehead. It was very annoying to him, from the constant discharge of matter of a peculiarly offensive character, which caused an excoriation of the skin, extending from the sinus to the inner canthus of the right eye. What added to his annoyance considerably was, that when writing or reading, with the head bent forwards, the matter would frequently drop on the paper or book, and he constantly required to wear a cap to cover all. He had consulted various eminer practitioners, one of whom advised him to have a piece of caustic inserted into the sinus. Latterly, he obtained the best surgical advice Edinburgh could afford, and was recommended to let the opening alone, as it might prove dangerous if interfered with. Dispairing of relief, and wishing to avoid society as much as possible, on account of the unsightly nature of the complaint, he about three years ago came to reside at Muckart, in this neighbourhood. Before describing the operation, I shall endeavour to give an account of the case, back as far as twenty-eight years ago, when the disease began to manifest itself. He states, that about twenty-eight years ago he had a most painful week of it when in Belfast. During his visit there, he suffered most severely from shooting pains in the forehead, which continued more or less until 1854, fifteen years ago. When residing in St Helen's, near Liverpool, the complaint reached a crisis. A swelling, the size of an egg, protruded from the forehead, and the eyebrows were also much swollen. It was lanced at this time, and it has continued to discharge matter ever since, but the pain he formerly suffered ceased.

On coming to me, after having been refused a chance of recovery, I must say I felt rather as if I should let it alone also; but after examining the sinus thoroughly, and making myself sure that I could remove the apparent cause of the discharge, I resolved on performing the operation.

I found, on introducing a probe into the sinus, that it proceeded in a direction from above backwards and downwards, deep into the frontal bone, the depth from the surface of the forehead to the extremity of the sinus measuring an inch and a half. The probe on passing to the cranium encountered dead bone, which felt as if loose. After manipulating the probe over the point of dead bone, it was found to enter a hole, which took a downward direction, but I was afraid to push the probe deeper, not knowing the thickness of bone that might be between the point of the probe and the brain.

Having told G. H. exactly what I had made out, and explained to him that I did not anticipate much danger, he at once put himself under my care, and came to Alloa on Monday the 10th of May to be operated on.

I was kindly assisted by my friend, Dr Duncanson of Alloa. We did not think it desirable to administer chloroform. I made an incision down to the pericranium of the shape of a Y, each portion of the wound from the centre being an inch in length. The centre of the wound corresponded to the situation of the sinus. I then reflected the three flaps made by the Y incision, of the whole thickness of the integuments, and exposed the bone covered with pericranium. The deep surface of the wound was sponged, and the probe applied to the exposed surface of pericranium. It entered a hole, and went in a direction backwards and downwards to the extent of nearly an inch. The probe was then moved in a direction forwards and outwards, and a portion of dead bone was seen to be loose, under the pericranium. The pericranium was cut with the scalpel around the margin, where the bit of dead bone was seen to move. A small elevator or punch was introduced at the hole (which was enlarged slightly by force), and the piece of dead bone tilted on its edge; it was then seized with a pair of dressing forceps and extracted. A quantity of pus was then sponged from the bottom of the wound, in the gap where the dead bone was lying. The wound was stitched with a few metallic sutures, and a dressing of a weak solution of carbolic acid applied. The operation took place on a Monday. The following day he complained of a little headache, and had slight swelling of the integuments surrounding both eyes, but the next day, Wednesday, the swelling began to abate, and he left for Muckart on the Saturday following. The wound healed entirely by the first intention, only a very little discharge from the seat of the old sinus. In a fortnight the sinus was entirely healed, not a vestige of matter, to use his own expression, coming out of it. His forehead is now quite healed, and he expresses himself as greatly satisfied with the operation.

The piece of bone extracted is three-quarters of an inch long, half an inch broad, and an eighth of an inch thick at its middle. The upper surface is irregular and rough, giving one the idea of its having been long undergoing the process of absorption. The inner surface is partly smooth, and of an undulating appearance, strongly suggestive of its being a portion of the inner table of the skull. Sir James Simpson, to whom I showed it, expressed himself favourable to this opinion; and Professor Turner, to whose inspection it was also submitted, thought this view very probable, though he would not vouch for its accuracy. The piece of dead bone had become completely detached, and was lodged in the cavity thus formed. The irritation of its presence kept up a constant discharge of offensive purulent matter, which found its way to the surface by the sinus on the brow. The bone itself was prevented from being discharged, from its being secured under the pericranium by the shell of new bone overlapping its edges, something like the way in which a watch-glass is retained in its place. Since writing this, I casually met E. H., and had an opportunity of again examining the seat of operation. The cicatrix is very slight, but on putting the finger on it, no bone can be detected at this part of the skull, showing that it was not a mere exfoliation of its outer table, but a deep exfoliation, most probably including the whole thickness of the frontal bone.

## ARTICLE VI.—*On the Action of the Cobra Poison.* By J. FAYRER, M.D., F.R.S.E., C.S.I.; Surgeon, Bengal Army; Professor of Surgery in the Medical College of Bengal.

(*Continued from page* 430.)

### ELEVENTH SERIES.

Present—Dr Francis, Dr Ross, Dr Fayrer, Dr J. Ewart, Dr D. B. Smith, Col. Showers, Mr W. F. Blandford, and Mr Sceva.

*16th Jan.* 1869.—The following experiment was made in the presence of the above gentlemen, with the view of testing an antidote described by Colonel Showers in his letter, and in the series of experiments performed at Gwalior in September 1868, and recorded in the November (1869) number of the Journal.

Colonel Showers having very kindly brought the man who administered the antidote to Calcutta, and expressed his wish that its effects should be tested, I accordingly, with the assistance of the above-mentioned gentlemen, proceeded to make the following experiments. The *kelaree* had been allowed to make any preparation that he deemed necessary on the day preceding, and in the morning of the day on which the experiment was performed.

It is right to note that he says, on the day of the experiments, that the antidote he has now with him is not the most potent one he knows, but that, owing to the absence of rain, he had not been able to procure it, as the drought had prevented the growth of the plant from which it is derived. He expressed his belief that the drug he had with him would prove sufficient to counteract the influence of snake-poison. Accordingly, a pariah dog was made over to him, and he administered to it a small piece of whitish-looking root, pounded and put in a piece of meat; this the dog, which was a full-grown pariah, ate readily. The *kelaree* next selected, from a basket of fresh cobras, a full-grown one of the spectacled variety, which was made to close its jaws three times in the dog's hind-leg, just at the fold of the skin of the thigh, and in the thigh. The dog showed signs of pain when bitten, but had not evinced the least fear of the snake when it was brought near him; it was probably the first he had seen. The experiment was performed in the usual place, and as follows:—

EXPERIMENT No. 1.—The antidote was given to the dog in a piece of meat at 11.55 A.M. of the 16th January. At 11.59 the dog was bitten by a full-grown spectacled cobra, of the variety known by the natives of Bengal as the *Gokurrah.* The snake was made, by a snakeman occasionally employed by me, to close its jaws three times at the fold of skin in the right thigh and in the thigh itself. We ascertained that the cobra had only one effective poison-fang, the other being broken, but with this he drew blood slightly.

*Noon.*—Dog licks the puncture; bitten leg weak; partially paralyzed. 12.1. P.M.—Dog lies down; shows indisposition to walk about. 12.3.—No pain apparently felt; is drowsy; refuses to be roused, and then walks about, but shows a tendency to lie down. 12.5.—Looks sleepy; roused, he walks, but soon lies down again. 12.13.—Breathing catching; is drowsy. 12.17.—Cannot walk without staggering; falls down when left alone; breathing hurried. 12.20.—Convulsed. The *kelaree* now applied some oil to the mouth and nostrils. 12.21.—Lies on the left side; universally convulsed; eyes glazed; pupils dilated, and insensible to light. 12.22.—Defecates; is pale and bloodless about the mouth and lips. 12.23.—Involuntary micturition. 12.27.—Respiration ceased. 12.31.—Dead. Ceased to breathe three minutes before heart ceased to beat. Bitten at 11.59. Died at 12.31. Death in 32 minutes.

The above notes were taken by Dr Ewart, who carefully watched and noted all the symptoms, and he adds the following remarks:—"There is evidence that death takes place through the nerve-centres, the heart continuing to beat after the respiration entirely ceases, which can only be through its own inherent irritability and its own ganglionic supply, and quite independent of the medulla, which, in all other respects, is *hors de combat* from three to four minutes before the heart actually ceases to pulsate."

This experiment, though unsuccessful in demonstrating the good effects of the reputed antidote, cannot be considered conclusive, as the man stated that it was not the most potent agent he was in the habit of using, nor, indeed, should I consider any single trial as proof either for or against the good effects of the drug, however it had resulted. Many and repeated experiments are necessary before any definite opinion can be formed on so important a subject; and, therefore, before recording one, it is expedient that the experiments should be made again; and as Colonel Showers has expressed a wish that the man should have further opportunity of exhibiting his antidote, I think that, on a future occasion, more certain results may be obtained.

Having studied the effects of the poison in many animals, and having formed conclusions as to the nature of the cause of death, I am sceptical on the subject of antidotes, and fear that the hopes of those who are most sanguine on the subject will not be realized. I am not the less anxious, though, to become acquainted with whatever may be of service in the treatment of snake-bites, whether prophylactic or therapeutic; and I shall be as glad to record any facts that tend to throw light on the subject, as to admit the potency of an antidote when I see it proved. Colonel Showers informs me that the *kelaree* makes the following objections to the experiment:—

*First*, That he was not fully prepared.

*Second*, That the antidote was not the most reliable one he knows of

*Third*, That the animal was bitten three times by the cobra.

With reference to these, I would remark that the time for the experiment was appointed some days previously; that he was invited to make any previous preparation he thought desirable; that the room in which the experiments were performed was placed at his disposal, and that certain snakes were also made over to him; that he locked himself in for some time the day before making his preparations; and that Mr Sceva was most careful to see that all he wanted was procured, and all his wishes carried out on the day of the experiment. The animal bitten was placed at his disposal to do what he liked with, and all his wishes were complied with to the minutest details. If, therefore, anything were wanting, the fault was his own.

Secondly, with reference to the antidote itself, it was suggested by himself, and he certainly expressed his belief that it would prove efficacious; or else why did he administer it?

As to the number of times the dog was bitten by the cobra, the snake was made to close its jaws in three places to ensure the injection of the poison; the first bite, at least, being doubtful.

I am quite satisfied that one bite, had it been fairly inflicted, would have been sufficient. Nor does the fact of there having been three punctures affect the question in this instance, for nothing occurred to show that the dog was in any way influenced by the so-called antidote. It succumbed with the same symptoms, and in about the same time, as other dogs that had not taken any antidote at all, and were bitten in the same way by a cobra.

The efficacy of the antidotes described by Colonel Showers shall be fully tested whenever the man who administers them declares himself to be ready and fully prepared.

EXPERIMENT No. 2.—At 12.13, a kid of about three or four months old was bitten three times on the hind-leg by the same cobra that bit the dog in the previous experiments. The object was, that in the event of the poison not taking effect in the dog, the activity of the poison might be demonstrated by its action on the kid. This, had the dog survived, and the kid died, would have been so far confirmative of the efficacy of the antidote. 12.15.—Leg weak; partially paralyzed. 12.16.—Bleating; staggers, but walks; lies down; defecation. 12.20.—Can stand, but the bitten limb is paralyzed. 12.25.—Almost paralyzed; convulsed. 12.29.—Convulsed. 12.34.—Heart beats 96 per minute; respiration almost gone. Eyes glazed; pupils dilated, insensible to light. 12.36.—Death. Heart beat for two minutes after respiration had ceased. Bitten at 12.13. Dead at 12.36. Death in twenty-three minutes.

The activity of the poison had no doubt been exhausted by the previous biting of the dog, otherwise a young animal like the kid would have probably succumbed more rapidly.

The following experiments were made to test the efficacy of the injection of liquor ammonia as an antidote. They will be repeated with various strength of the ammonia in solution:—

Experiment No. 3.—At 12.36, a pigeon was injected in the thigh with cobra poison recently taken from the living snake. Two drops were inserted into the muscles of the thigh with the hypodermic syringe. 12.37.—Is affected by the poison; staggers; is slightly convulsed and drowsy. 12.39.—Droops and falls over, but is able to walk when roused. Ten drops of liquor ammonia, diluted with three times the quanity of water, injected by means of the hypodermic syringe into the same thigh. 12.40.—Very drowsy; rests the point of the beak on the ground; legs stretched out with a convulsive quivering motion; head falls over. 12.40,30.—Gasping respiration. 12.41.—Dead. Death occurred in four minutes—rather a long period for so small and sensitive a creature as the pigeon; but the quantity of poison was small (2 drops), and all that was not thoroughly inserted. In this instance, I do not believe that any beneficial effect was caused by the injection of the ammonia.

Experiment No. 4.—12.54.—A full-grown pariah dog had the femoral vein exposed, in order that the solution of ammonia might be readily injected with the hypodermic syringe; it was then bitten in the opposite thigh by a fresh full-grown spectacled cobra. 12.56.—Dog walks with staggering gait; the bitten limb is weakened. 12.57.—ʒi. of liquor ammonia, thrice diluted with water, injected into the femoral vein. 1 P.M.—Limps on the bitten leg, and lies down. 1.3.—Leg drawn up; slightly drowsy. 1.6.—Looks more drowsy; sits down. 1.7.—Made him swallow ʒii. of liquor ammonia, well diluted with six parts of water. 1.9.—Pupils dilated; lies down exhausted. 1.12.—Lying flat on the left side. 1.15.—Staggers. When roused, is not quite so drowsy; refuses water; lies down, whining and moaning; stretches out the legs as though in pain. 1.24.—Pupils much dilated; froths at the mouth; very weak in the hind-legs, but more so in the bitten than the other limb; still moaning. 1.26.—Paralyzed completely in the posterior extremities; jerking movements of the head. Respiration, 44; pulse, 100. 1.29.—Respiration, 28; pulse irregular, fluttering, and difficult to count; convulsed; is very restless; convulsive movements of the diaphragm; gasping; spasm of diaphragm; pupils dilated, insensible to light. 1.30.—Struggled and changed the position to the other side. Respiration, 12 in the minute; pulse very irregular; cannot be counted from subsultus tendinum. 1.34.—Defecations. Heart beats 104 in the minute; fluttering irregular pulse; respiration has ceased; muscular twitchings. 1.37.—80 beats of heart in the minute; muscular twitching. 1.38.—Heart beats faint, slow, but perceptible. 1.38,15.—Heart ceased to beat about four minutes after respiration ceased. Dead in forty-four minutes and fifteen seconds. Death was rather later in this case than usual in the case of a full-grown pariah dog bitten by a full-grown and fresh cobra. It would appear, therefore, that the ammonia may have been so far beneficial. The benefit, however, is very small; but further experiments may prove that, given in larger quantities and more frequently, it may be of service.

## ARTICLE VII.—*On the Condition of the Optic Disk and Retina in Acute Iritis.* By ROBERT P. OGLESBY, Assistant-Demonstrator of Anatomy in the Leeds School of Medicine.

FOR a considerable time past I have been collecting evidence regarding the condition of the retina and optic disk in the acute forms of iritis; and from the result of such investigations I have been induced to record the experience thus obtained.

As yet I cannot adduce sufficient evidence to give with certainty anything approaching valuable statistical information as to the frequency of the occurrence of these lesions, on account of the great difficulty experienced in subjecting patients to proper ophthalmoscopic examinations; either because of their inability to submit to such an examination from extreme intolerance of light, or from the condition of the pupil. Nevertheless, I hope that by dwelling briefly on this subject, I may draw that attention to it which I believe it to deserve.

Effusion of lymph into the retina, peripapillary œdema, or optic neuritis, are frequent concomitants, if not a condition always present at some stage of the disease. I may state that I have seen neuritis supervene upon well-marked specific iritis which had existed for five days; and that I have watched the commencement of neuro-retinitis in a case of an exactly similar kind, with this difference, that the iris had been inflamed for a longer period.

In the few cases of iritis which I have met with of a non-syphilitic character, the disk and retina were free from disease; but examples have not been sufficiently numerous to warrant me in concluding that such lesions only occur in specific cases. The continued impairment of vision in these cases, after the disease of the iris has abated or is cured, is, I believe, chiefly due either to inflammation of disk or retina, or a degree of serous effusion into the one or the other, of so slight a nature as to escape other than very close ophthalmoscopic examination.

In such cases contraction of the field of vision will be found often in a marked degree. In these cases, mercury proves highly beneficial. In a letter on the subject from Mr W. Spencer Watson (who most kindly gave me valuable information), the following passage occurs:—" I am convinced, however, that in some instances the optic disk and retina are affected during the acute stage of iritis, from the condition in which I have found the retina at subsequent stages. I have notes of a case which occurred in 1863. It was a case of dotted opacity of the cornea with iritis—probably specific. Distinct traces of retinitis were made out after the acute symptoms had subsided. Similar cases have every now and then occurred to me."

That the occurrence of retinitis and optic neuritis during acute iritis is familiar to most ophthalmologists, I have little doubt; but as I have failed, in a long search through the literature on the subject, to find any definite account of these pathological changes, I venture to put pen to paper, trusting that by so doing it may lead to a more minute investigation into these interesting lesions of nerve structure.

Appended are the notes of two well-marked cases which I copy from my case-book.

Case I.—G. M., æt. 20, cabman, states that he never suffered from any affection of the eyes previously. A year ago he contracted syphilis. Five days ago the right eye became painful. There was intolerance of light, vascularity and lachrymation. A tubercle of lymph rested on the upper and inner part of the iris, which was discoloured. The pupil was considerably dilated by the use of atropine drops applied the day before. The ophthalmoscope showed a curious roughened (?) condition of the disk. Four days subsequently there was a sudden impairment of vision. There was now marked congestion of the disk, and the veins were decidedly tortuous. Several days elapsed before the patient again presented himself for examination. To all external appearance the iritis was cured, but vision had become alarmingly worse. On examination, the disk presented a well-marked example of neuritis. The inflammation existed for nearly five weeks, then the effusion began slowly to disappear. After the lapse of several weeks, steady improvement of vision set in, and is now nearly normal.

Case II.—E. T., a single woman, æt. 27, seven years ago, contracted syphilis, from which she has suffered seriously. Six weeks ago, she for the first time felt the right eye to be weak, and it has grown considerably worse since. She was then under the care of Mr Libbey of Leeds, to whose kindness I am indebted for this and other valuable ophthalmic cases of great interest. There was inflammation of the right iris, but the pupil was considerably dilated under the influence of atropine, which had been applied to the eye for several days past. Vision was limited to shadows. The opthalmoscope showed that both retina and disk were suffering (neuro-retinitis). I am sorry that, from the waywardness of my patient, I was unable to watch the case longer; but as I could not promise her immediate relief, she left me to seek it elsewhere.

## ARTICLE VIII.—*Notes of Cases of Scarlet Fever.* By WILLIAM KERR, Surgeon, Braehead, Galt, Canada West, Corresponding Member of the Medical and Physical Society of Calcutta, and of the Medico-Chirurgical Society of Glasgow.

DR COPLAND has stated that "the treatment of scarlet fever is unsatisfactory, and, in the worst cases, most unsuccessful."[1] In the greater part of the following instances, I trust that this opinion may be reversed. I do not venture to say what proportion such will generally bear to the whole number, but I cannot allow myself to believe that they will be exceptional. The remedy is the same as I have recommended in this Journal for dysentery,[2] omitting opium, which is to be added only when diarrhœa is present. It therefore consists of the powdered leaves of Stramonium, Dulcamara, Sium lineare, Cicuta maculata, and Conio-selinum canadense, together with digitalis, squills, or anhydrous bisulphite of soda, digitalis, or squills being in the proportion of a half part, bisulphite of soda in the proportion of two parts, the others in that of one each. To suit differences of age, of constitution, and of the prevailing epidemic, three combinations are formed out of these; one containing digitalis and the five ingredients which precede it in the list generally best adapted to adults; a second with squills substituted for digitalis, generally best adapted to infants and children; and a third, with bisulphite of soda taking the place of digitalis or squills, adapted to all ages.

I have often been asked—Why use so many ingredients, especially why put together several not previously known in medicine? Suffice at present to say, that numerous trials were made at first with henbane and camphor, and afterwards with various combinations of the different narcotics of the Materia Medica; but these not producing the desired measure of success, aid was sought from the unrecognised wild plants of the woods and swamps. In no instance was a change or addition made till want of success demanded it; the change never consisted of more than one ingredient, and was always tried in as many patients as possible. In this way the necessity for, and benefit from, each was tested with all the care I was able to exercise. The result of this search, comprising an examination of thirty-two plants or their products, and extending over five to six years, was a conviction, that though a medicine composed of a few of the ingredients specified might in some instances be adequate to cure, yet that the proportion of failures was very much reduced when all the ingredients specified were employed. The acknowledged necessity for more than a single nutritive principle in diet is suggestive that we may be wrong in relying upon a single medicine in disease. As far back as the year 1812, Dr Paris, in his "Pharmacologia," insisted that increased power is gained by combination of medicines of the same class; but, possibly owing to the discovery of the vegetable alkaloids, simplicity in prescription has prevailed, and the views of Dr Paris have been overlooked.

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[1] In the year 1864 there were 40,000 deaths from scarlet fever in the United Kingdom.

[2] June 1865, and September 1867.

The medicine now recommended is founded upon the principle that power is gained by combination, each ingredient apparently possessing some property not contained in the others, the combined effect of these properties being necessary to combat the disease. In accomplishing this object, another of considerable importance has been obtained; owing possibly to the small quantity of each ingredient, in general no deleterious effect is produced, and the patient is sensible only of relief. In the various affections in which this medicine, or combination of medicines, has been tried by my medical friends or myself, its action is to heal ulceration of the mucous membrane, and the vascular and tender state which precedes it; often in the course of half an hour to induce refreshing sleep, gradually to bring about better digestion, and in scarlet fever to keep the bowels sufficiently open without causing uneasiness, and usually without requiring the aid of any other aperient. The digitalis combination, in some constitutions, produces delirium, dimness of vision, and giddiness; in others, pain or oppression in the region of the heart. The squill gives rise to no unpleasant effect, and, as a general rule, is suited to children; it may, however, fail to relieve. Possibly the combination having bisulphite of soda may suit constitutions not relieved by either of these, but of this my experience is still limited.

The mucous membrane being affected in dysentery, and also in scarlet fever—diarrhœa likewise being a not unfrequent attendant of the latter—I entertained a hope that a medicine which has now for many years been eminently beneficial in dysentery,[1] in the hands of my medical friends as well as my own, would also be valuable in scarlet fever. Excepting, however, a few slight cases at an early stage of the investigation, I saw none till the combination had reached its maturity. Ten years ago a young woman, on the second day of the eruption, had vomiting of fluid like coffee-grounds—always regarded as a fatal symptom—together with ulcerated sore throat, headache, and sleeplessness. In a few hours, after two doses of the combination of six grains each (without opium), she was asleep, and every symptom relieved; recovery was rapid.

Dr Philip, of Galt, in a communication to me, says: "In eleven severe cases of scarlet fever, in the autumn of 1860 and spring of 1861, I gave to some the digitalis, and to others the squill combination. Whether administered for the first time on the second day, or not till the fourth or fifth, relief uniformly commenced in a few hours at farthest. Soon after the first or second dose sleep ensued, pain and vascularity of the fauces abated, frequency of the pulse and feverish heat diminished, and vomiting ceased. Diarrhœa and vomiting were present in a few of the worst cases, but were always stopped by one or two doses containing a minute quantity of opium.[1] A crisis might therefore be said to have taken place, and the disease to have been cut short, within twelve or twenty-four hours from the administration of the first dose; the eruption, notwithstanding, continuing the usual time. In some, there were glandular swellings beneath the ears; and though I dreaded suppuration in one or two scrofulous children, the swellings gradually disappeared. No patient, whether slightly or severely affected, was afterwards seized with anasarca, and all recovered except one of my earliest, to whom, from want of confidence at that time, the medicine was not administered till after the failure of ordinary remedies; even then relief and benefit were most marked, and the patient died from exhaustion, and not strictly from scarlatina. To my earlier patients I likewise gave chlorate of potash, but gradually trusted altogether to your remedy." Since 1861 scarlet fever has not been epidemic in Galt; but in the cases which have occurred to him, Dr P. has given either the digitalis or squill combination, and the results corresponded with his experience, as now stated.

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[1] On the representation of gentlemen connected with the Calabar Mission, the Secretary of State for India sent a quantity of my remedy for dysentery to Calcutta; and a quantity was also forwarded to Bombay at the request of D Arnott, Inspector-General, so that medical gentlemen in India desirous making a trial may obtain it at these places.

From the notes of Dr Mackintosh of Hamilton, I extract the following cases:—

No. 1.—1863.—A girl, eight years of age, was seized in school, and sent home complaining of sore throat; she was much flushed, and vomited repeatedly. During the night, diarrhœa, delirium, and high fever were superadded. Next morning, when first visited by Dr M., her face was flushed and congested, throat inflamed, almost purple, deglutition difficult, pulse very frequent; one grain and a half of the digitalis combination was directed to be given every three hours. She was quickly relieved, and spent a good night; next day, the third of the illness, all the symptoms were considerably improved, and on the succeeding day she was almost well. Dr M. remarks that this was a very severe attack, and the action of the medicine speedy and most satisfactory.

Nos. 2 and 3.—Two severe cases, which occurred at this time; both were rapidly relieved and cured. Of one of these it is said that she was first seen on the third day, and in four hours after two doses was much better in every respect.

No. 4.—This patient was not visited till the disease was far advanced, and extensive abscesses had formed in the neck. Died.

No. 5.—18*th October* 1864.—A stout boy, aged four years; initiatory symptoms severe; digitalis combination prescribed. After some days' treatment, all the symptoms were favourably modified; the recovery was good, without sequelæ.

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[1] For adults one part, and for infants half a part, of opium to five and a half of the digitalis or squill, and seven of the bisulphite of soda combination.

Nos. 6 and 7—*23d October.*—Two children, aged respectively three years and fourteen months. The former, a very severe case, was under treatment for fourteen days, but ultimately did well under the digitalis combination, followed by quinine and iron. The latter, mild; after each dose sounder sleep always ensued; cured in a few days.

Nos. 8 and 9.—*25th November.*—Two children, aged seven and five years; the former mild, and recovery rapid; the latter as severely and dangerously affected as Dr M. ever witnessed; was nearly two weeks under treatment, but had a good recovery under the digitalis combination, followed by quinine and citrate of iron.

No. 10.—*27th November.*—A child four years of age; moderately severe attack; did well under the same treatment.

Nos. 11 and 12.—*1st December.*—Two children, aged three and five years; the former, a neglected case, not seen till very late in the disease, died; the latter did well under treatment with the digitalis combination.

No. 13.—*19th December.*—A boy aged twelve years; a severe attack; treatment as above; recovered.

No. 14.—A girl aged three years; a pretty severe attack; did well under the same treatment.

Nos. 15—20.—Six of Dr Mackintosh's own children were almost simultaneously seized, three slightly, and three severely; indeed, so severely, that Dr M., with ordinary remedies, would have looked upon the attacks as necessarily fatal. He trusted all to the digitalis combination, no other medicine, except gargles, being given.

Dr M., in reply to a letter requesting a statement of his experience up to the present time, says: "For the last four years I have kept no record of cases, on account of the uniform success attending the administration of your medicine; the result, to the best of my recollection, having been in every instance successful. In malignant cases, I also kept the apartment well ventilated, and the air slightly impregnated with sulphurous acid."

Dr Howitt of Guelph, in five severe cases, gave the digitalis combination; all were speedily relieved and cured.

A child of Dr Ogden, lecturer on Materia Medica, Toronto, aged eighteen months, was seized with scarlet fever, which at first was mild, but as the eruption was passing away the fauces became swollen, livid, and ulcerated; at the same time, cough and oppressed breathing from the secretion of viscid mucus came on; she was restless, and slept little. There was a copious discharge of matter from the mouth, and in a lesser degree from the nostrils and meatus auditorius externus, which, possibly owing to the acrimony of the matter, excoriated the lips, nostrils, and lobes of the ears; there was also diarrhœa. Chlorate of potash was the medicine chiefly relied upon; and on the accession of diarrhœa, Dover's powder was added, but no improvement took place. The danger being evidently great, and Dr Ogden, having previously successfully treated one or two cases of scarlet fever with the squill combination, now gave it along with opium. Diarrhœa and the bronchial symptoms were at once relieved, better sleep was enjoyed, the ulcers began to heal, and recovery went on from almost hopeless symptoms.

In February 1864, Dr Macintyre of Hespeler, C. W., applied to me under the following circumstances:—Three children of a family had died of scarlet fever, and a fourth, nine years of age, was dangerously ill. It was the seventh day of the disease, pulse 150, glands beneath the angle of the jaw swollen, and the throat very painful. In fifteen minutes, after a dose of the digitalis combination, was asleep; on awaking, two hours afterwards, pain was evidently lessened, and the pulse had fallen to 140. Four doses were given daily, recovery was rapid, and the swollen glands diminished, without suppurating.

The following fatal case occurred in my own experience:—An unhealthy child, two years of age, who had just recovered from a severe attack of measles, thirty-six hours after seizure had a pulse at 180, eruption dusky red, ulcerations in the back of the fauces, and breathing oppressed from the secretion of viscid mucus. She had been fretful and without sleep from the commencement. In ten minutes, after two grains of the squill combination, was asleep, and by three such doses daily, slept without oppression in a comparatively comfortable manner nearly the whole of the following six days, the stuffing and tenderness of the nostrils and the phlegm in the fauces becoming less. On a few occasions the administration of a dose was inadvertently postponed some hours, when restlessness took place, which, on a dose being given, was always allayed, and in a few minutes the child was asleep. Neither was the benefit confined to the promotion of sleep; in six hours after the first dose, swallowing, which previously had been very painful, became comparatively easy, feverish heat declined, eruption became brightened, and the child passed through the disease with little suffering. One unfavourable symptom continued, notwithstanding—the pulse hardly abated in frequency. At an early period, two discharges of half-digested blood took place, producing great weakness, and indicative of extreme danger. Petechiæ gave a similar indication. She was supported by brandy, and, as soon as appetite began to return, by nutritious food; I may also add, by long and sound sleep during the whole treatment. The period of desquamation had passed, but her strength did not rally, and she died exhausted on the eighteenth day, the only diseased appearance, as shown by a post-mortem examination, being slight ulceration of the fauces, still unhealed.

In contrast with the persistent frequency of the pulse in the preceding case, caused, I apprehend, by weakness, is the following:—The medicine was first given to the patient, a boy of ten years, on the second day of the eruption; the pulse was then 156; on the third day 120; and on the fourth, when the eruption was at its height, 96; all the symptoms which indicated danger having diminished in severity along with the decline of the pulse.

*25th May* 1868.—A child, fourteen months old, previously attended by another practitioner, who this morning had been under the necessity of going to a distance, and who had treated her with ordinary remedies. The symptoms having this day become much worse, I visited her at 5 P.M. The pulse was 188, breathing difficult, and swallowing painful; glands on both sides of the neck, especially on the left, swollen; tonsils inflamed and swollen, and a white slough on the left; eruption nearly gone; lips and cheeks somewhat livid; space around the mouth pale; restless, constantly pushing her mother, on whose knees she was lying, with her hands and feet; drowsy, but unable to sleep on account of difficult breathing. I gave three grains of the bisulphite of soda combination, and waited an hour to witness the effect. At the end of this time she was sensibly easier, and sleeping somewhat soundly.

9 P.M.—A second dose of three grains was given an hour ago; at present pulse 148, swallows with little difficulty; breathing easier; sleeping more calmly; bowels moved once, the first time to-day.

*26th.*—10 A.M., Sixth day.—Last night slept well, except for a short time about 4 A.M., when the effect of the medicine began to wear off, but another dose restored sleep and tranquillity; slough gone; glands less swollen and less painful; swallows easily; pulse 144.

9 P.M.—Had a fourth dose at 11 A.M., and a fifth at 7 P.M. Bowels moved once to-day.

*27th.*—Seventh day.—Slept well last night; sixth dose at 5 A.M. Appears now to have made up lost sleep, as a dose is not followed by drowsiness as formerly.

*28th.*—Though the swelling of the glands has disappeared, this forenoon pus commenced to flow from the left ear.

*1st June.*—Tincture of the digitalis has been dropt into the ear daily since last report, and pus is now little more than visible. Bisulphite of soda combination was given for five days in whole.

*6th May.*—10 A.M.—A sister of the preceding, three years of age, was seized during last night with vomiting. At present is unwilling to swallow on account of pain of the throat, which is reddened internally, but there are neither slough nor swelled glands; breathing is noisy; she tosses in bed, her eyes are half open, and her appearance indicates distress; pulse 160. Skin, especially of the face and neck, red. Three grains of the bisulphite of soda combination were given, and in fifteen minutes she was asleep, and breathing more easily.

9 P.M.—Two hours ago, restlessness having come on, a second dose was given, which was followed by tranquillity; indeed she has

slept almost the whole day. At present swallows easily, pulse about 150. Bowels not yet moved.

27*th.*—10 A.M.—A third dose was given last night, and a fourth early this morning. Slept well; no soreness of throat; eruption abating; at present is playing with her toys in bed.

1*st June.*—Well. Bisulphite of soda combination was given for five days.

It may be remarked of both patients, that from a very short time after the first dose there was little suffering, evidenced by continuous comfortable sleep, and diminished pain in swallowing.

An elder brother had an attack of scarlet fever, which ran its course very mildly. Bisulphite of soda combination was given from the commencement, and continued five days, each dose giving relief.

Another brother, who had had the disease about the beginning of May, had been out of bed, and often out of doors, for a fortnight, when he began to complain of pain in swallowing; at the same time, the glands at the angle of the jaw were discovered to be swollen and painful. Six grains of the bisulphite of soda combination were given twice a day. In twenty-four hours he could swallow easily, and in ten days the glands had returned to nearly their natural size—a common result likewise under the digitalis or squill combinations, as may be seen in several of the preceding cases. I am glad to say that in no case has deafness followed,[1] and anasarca in only one.

The reader may be disposed to ask, Why in the last four cases did I depart from the treatment employed in the others, and adopt a combination containing bisulphite of soda? My answer is, that failures occurring to myself and others, in some instances of affections of the mucous membrane, led me to believe that a third combination was required to meet varieties of constitution in individuals, and changes in the epidemic constitution of disease. At present I can only say that the trials—not yet numerous, however—with the bisulphite of soda combination have been very encouraging. Probably this combination will be found to be suitable to many cases to which the digitalis or squill would have been equally beneficial; but it is possible that some may occur which may require it in preference.

When these pages were going to the press, I received the following from Dr Vardon, Hawksville, C.W.:—"I have taken notes of thirty-five important cases of scarlet fever which have occurred within the last two months in the epidemic prevailing in this locality, but want of leisure compels me to give merely the result of my observations. I deem myself fully justified in stating that the combination used—that containing bisulphite of soda—when administered in the early stage, produced in twelve to eighteen hours a decided amelioration of the symptoms; even where pain in the throat had been so severe that patients swallowed with great difficulty, they were so much relieved as to be able to drink with comfort, the pulse, losing its tension, having become fuller and less frequent, and the skin having also become cooler and more moist; in fact, the disease was conquered. In several of the severe cases—*I had none of the malignant variety*—there was diarrhœa; in treating these I added a small quantity of Dover's powder to each dose. I have seen no ill effect from the medicine, and have not lost a patient."

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[1] According to statistical tables, scarlet fever adds from one-fifth to one-third to the number of the deaf. This is merely from memory, and perhaps, according to recent statistics, not accurate.

A month afterwards Dr Vardon writes as follows:—"The epidemic of scarlet fever in this locality has now terminated. I have treated 43 cases with the bisulphite of soda combination, to which, when the patient was plethoric, I added a portion of the digitalis. The greater part would, in all probability, have got well under ordinary treatment; nine, however, had *very severe* attacks, yet I am happy to say that every one has recovered.

"To afford a better idea of the severity of some of the onsets, and of the effect of treatment, I shall give two which were complicated with convulsions.

"A boy, aged eight years, on the 14th April was seized with convulsions, and had five fits before I reached his residence at 5 P.M. His pulse was then 140, skin very hot, constant vomiting, conjunctiva injected, delirium, the throat so much swollen, and, together with the nares, so filled with frothy mucus, that respiration was hurried, and performed with difficulty; eruption well out. I ordered a warm bath, mustard to the throat and epigastrium, and gave ten grains of the bisulphite of soda combination, to be followed by eight grains every six hours.

"*15th.*—After swallowing the first dose yesterday, an hour elapsed without vomiting, which has ceased altogether since the second; during the night slept five hours at one time, and had several short naps; swelling in throat very much lessened; skin moist; pulse 100; bowels once moved.

"Next day, his father called upon me and reported him well. Three days afterwards I was requested to visit another child in the same family, and found my late patient sitting in bed, free from fever, and out of danger.

"*16th April.*—A child, aged eighteen months, was seized with convulsions, which had continued eleven hours before my arrival; throat very much swollen, and clogged with mucus; fluids in part returned by nostrils; no eruption. Ordered warm bath, followed by friction with warm flannels; gave five grains of the bisulphite of soda combination with two of the digtalis, and ordered a similar dose every six hours.

"*17th.*—After second dose, slept for four hours; has had no more convulsions; throat very much better; can swallow with ease; scarlet eruption general over the body; skin moist.

"Recovery was completed in four days.

"In only one instance did anasarca follow recovery from scarlet fever."

I have so often mentioned sleep following the administration of the combinations, that I may say that sleep thus procured seems to come rather from relief to uneasiness or pain than from a directly soporific effect; because, when distress has terminated, and the patient no longer requires unusual length of sleep, drowsiness no longer follows a dose. By consulting Dr Brown's case in my first paper on Dysentery, June 1865 of this Journal, it will be seen that sleep from opium in that instance nearly approached to narcotism, but sleep from the digitalis combination was sound and refreshing. I may say, that in all instances which have come under my notice, such is the character of the sleep so procured. Moreover, in scarlet fever, the respiration, previously oppressed from the presence of viscid secretion in the fauces, becomes easy while the patient is sleeping; and by a repetition of doses, sleep may be maintained as long as exhaustion caused by the disease demands it.

The cases related in this paper are at variance with the received opinion that scarlet fever must run its course, and that the utmost which medical treatment can accomplish is to mitigate symptoms, though very often failing to do even this. I trust that I may venture to say that these cases show that the disease may be arrested in any part of its course, if no complications have formed. Referring to the reports, the patient, though previously tossing and restless, was asleep in an hour from the first dose; in a few hours pain of the throat was lessened, and, consequently, swallowing was easier; by this time the pulse was found to be less frequent, and each succeeding day generally witnessed a progressive decline both of the pulse and of feverish heat. By what name are such changes to be called, but an arrest of the disease?

*Additional Case treated by Mr Kerr.*—W. H., 15 years of age. On the evening of the 29th May 1869, felt unwell, had no appetite for tea; complained of sore throat and headache, and by bedtime was observed to rave. He passed a restless night, and next morning I visited him. His face, breast, and arms were then covered with a bright scarlet eruption, fauces inflamed, and a considerable slough on both tonsils; swallowing painful; tongue covered with a thick white fur, pulse 108. Seven grains of the bisulphite of soda combination were prescribed to be given every three hours.

31*st.*—7 A.M.—Has taken the medicine as directed. Slough gone from one side and lessened on the other; tongue beginning to clean; swallowing easier; pulse 96. Medicine to be continued at the rate of seven grains every four hours, and a gentle laxative given immediately.

*June* 1.—Has enjoyed a great deal of sleep; slough gone; tongue clean; swallows easily; has some appetite; pulse 76; eruption general, of a bright red.

The cure was completed with unusual rapidity.[1]

## Article IX.—*On Aggregation in the Dublin Lying-in Hospital.* By J. Matthews Duncan, M.D., Vice-President of the Medico-Chirurgical Society.

In this short paper, I propose to inquire what information can be derived from the statistics of the Lying-in Hospital of Dublin, as to the influence of aggregation of lying-in women on their mortality. In the records of that institution, there have been accumulating, for above 100 years, facts bearing upon this point. These facts are embodied in a table, showing the number annually delivered, and the corresponding yearly mortality. This table may be got in various places. I shall use that which is to be found at p. 30 of Dr Evory Kennedy's little book, entitled, "Hospitalism and Zymotic Diseases," etc.

I suppose no one doubts that overcrowding the wards of a maternity or of any other hospital, is one of the most certain causes of danger and death to the inmates. On this great subject I do not now propose to say a single word. I mean to show what information can be derived, with certainty, from the records of the Dublin Hospital, as to the influence on mortality resulting from the bringing together of different numbers of lying-in women. In other words, does the experience of the Dublin Hospital show that there has been, in bringing together for lying-in a small number, greater mortality than in bringing together a large number for the same purpose?

This inquiry will be valuable with a view to throwing light on the present discussion regarding hospitals. The information derived will, for reasons already given elsewhere, be more reliable for this purpose than that obtained by studying corresponding amputation statistics. Further, the information derived will be, in my opinion, valuable, so far as it goes, to a degree which no other statistical information can approach, because of the long time and the large numbers involved in this great hospital's experience —above 190,000 cases, in 113 years. These data I here give in a tabular form:—

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[1] The medicine as recommended in this paper may be obtained from Messrs Duncan, Flockhart, & Co., chemists, Edinburgh, and Messrs Murdoch Brothers, chemists, Glasgow.

TABLE I.—*Showing Death-Rate per Thousand of Patients in Dublin Lying-in Hospital for the Years* 1757 *to* 1868 *inclusive.*

| Year. | Patients. | Deaths. | Per 1000. | Year. | Patients. | Deaths. | Per 1000. | Year. | Patients. | Deaths. | Per 1000. | Year. | Patients. | Deaths. | Per 1000. |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| 1757 | 55 | 1 | 18·1 | 1785 | 1292 | 8 | 6·2 | 1813 | 2484 | 62 | 24·9 | 1841 | 2025 | 23 | 11·4 |
| 1758 | 454 | 8 | 17·6 | 1786 | 1351 | 8 | 5·9 | 1814 | 2508 | 25 | 9·9 | 1842 | 2171 | 21 | 9·7 |
| 1759 | 406 | 5 | 12·3 | 1787 | 1347 | 10 | 7·5 | 1815 | 3075 | 17 | 5·5 | 1843 | 2188 | 22 | 10·1 |
| 1760 | 556 | 4 | 7·2 | 1788 | 1469 | 23 | 15·7 | 1816 | 3276 | 18 | 5·5 | 1844 | 2176 | 14 | 6·4 |
| 1761 | 521 | 9 | 17·3 | 1789 | 1435 | 25 | 17·4 | 1817 | 3473 | 32 | 9·2 | 1845 | 1411 | 35 | 24·8 |
| 1762 | 533 | 6 | 11·3 | 1790 | 1546 | 12 | 7·8 | 1818 | 3539 | 56 | 15·8 | 1846 | 2025 | 17 | 8·4 |
| 1763 | 488 | 9 | 18·4 | 1791 | 1602 | 25 | 15·6 | 1819 | 3197 | 94 | 29·4 | 1847 | 1703 | 47 | 27·6 |
| 1764 | 588 | 12 | 20·4 | 1792 | 1631 | 10 | 6·1 | 1820 | 2458 | 70 | 28·5 | 1848 | 1816 | 35 | 19·3 |
| 1765 | 533 | 6 | 11·3 | 1793 | 1757 | 19 | 10·8 | 1821 | 2849 | 22 | 7·7 | 1849 | 2063 | 38 | 18·4 |
| 1766 | 681 | 3 | 4·4 | 1794 | 1543 | 20 | 12·9 | 1822 | 2675 | 12 | 4·1 | 1850 | 1980 | 15 | 7·6 |
| 1767 | 664 | 11 | 16·6 | 1795 | 1503 | 7 | 4·7 | 1823 | 2584 | 59 | 22·8 | 1851 | 2070 | 14 | 6·8 |
| 1768 | 655 | 16 | 24·4 | 1796 | 1621 | 10 | 6·2 | 1824 | 2446 | 20 | 8·2 | 1852 | 1963 | 11 | 5·6 |
| 1769 | 642 | 8 | 12·5 | 1797 | 1712 | 13 | 7·6 | 1825 | 2746 | 26 | 9·5 | 1853 | 1906 | 17 | 8·9 |
| 1770 | 670 | 8 | 11·9 | 1798 | 1604 | 8 | 4.9 | 1826 | 2440 | 81 | 33·2 | 1854 | 1943 | 37 | 19·0 |
| 1771 | 695 | 5 | 7·2 | 1799 | 1537 | 10 | 6·5 | 1827 | 2550 | 33 | 12·9 | 1855 | 1060 | 35 | 33·0 |
| 1772 | 704 | 4 | 5·7 | 1800 | 1337 | 18 | 13·5 | 1828 | 2856 | 43 | 15·1 | 1856 | 1600 | 25 | 15·6 |
| 1773 | 694 | 13 | 18·7 | 1801 | 1725 | 30 | 17·4 | 1829 | 2141 | 34 | 15·9 | 1857 | 1509 | 33 | 21·9 |
| 1774 | 681 | 21 | 30·8 | 1802 | 1985 | 26 | 13·1 | 1830 | 2288 | 12 | 5·2 | 1858 | 1084 | 30 | 27·7 |
| 1775 | 728 | 5 | 6·9 | 1803 | 2028 | 44 | 21·7 | 1831 | 2176 | 12 | 5·5 | 1859 | 1389 | 21 | 15·1 |
| 1776 | 802 | 7 | 8·7 | 1804 | 1915 | 16 | 8·4 | 1832 | 2242 | 12 | 5·4 | 1860 | 1404 | 26 | 18·5 |
| 1777 | 835 | 7 | 8·4 | 1805 | 2220 | 12 | 5·4 | 1833 | 2138 | 12 | 5·6 | 1861 | 1135 | 59 | 51·9 |
| 1778 | 927 | 10 | 10·8 | 1806 | 2406 | 23 | 9·6 | 1834 | 2024 | 34 | 16·8 | 1862 | 800 | 58 | 72·5 |
| 1779 | 1011 | 8 | 7·9 | 1807 | 2511 | 12 | 4·8 | 1835 | 1902 | 34 | 17·8 | 1863 | 1228 | 32 | 26·1 |
| 1780 | 919 | 5 | 5·4 | 1808 | 2665 | 13 | 4·9 | 1836 | 1810 | 36 | 19·9 | 1864 | 1184 | 26 | 21·9 |
| 1781 | 1027 | 6 | 5·8 | 1809 | 2889 | 21 | 7·3 | 1837 | 1833 | 24 | 13·1 | 1865 | 1332 | 30 | 22·5 |
| 1782 | 990 | 6 | 6·1 | 1810 | 2854 | 29 | 10·2 | 1838 | 2126 | 45 | 21·2 | 1866 | 1074 | 40 | 37·2 |
| 1783 | 1167 | 15 | 12·8 | 1811 | 2561 | 24 | 9·4 | 1839 | 1951 | 25 | 12·8 | 1867 | 1146 | 40 | 34·9 |
| 1784 | 1261 | 11 | 8·7 | 1812 | 2766 | 43 | 15·6 | 1840 | 1521 | 26 | 17·1 | 1868 | 1022 | 39 | 38·2 |

These data have, as might be expected, been already used by students of the influence of aggregation. Especially, Dr Evory Kennedy has availed himself of them; and trusting to them almost alone, he tries to establish the proposition, "that the generation and absorption of this contagion (of puerperal metria) is in a direct proportion to the number of parturient females cohabiting in a given number of feet of atmospheric space at their parturient period, or who breathe the same atmosphere when lying-in."—(*Hospitalism*, etc., p. 25.) So important does Dr Kennedy consider this proposition, that he calls it his Redan proposition; and it truly is so. If it is secure, his cause is gained, and *vice versa.*

Dr Kennedy has made no proper analysis of the table on which he founds his proposition, but trusts to what he calls a bird's-eye view of it, and to a running commentary on its figures.—(*Hospitalism*, p. 118.) Both of these processes of taking the evidence derivable from the table, are, to me at least, quite novel. To them Dr Grimshaw adds another "upon the system of curvilinear test."—(*Hospitalism*, p. 123.) All three methods prove nothing whatever—show nothing towards proof of the proposition. They are not calculated, as they stand, to show anything. There is only one way of extracting the evidence from the table, and Dr Kennedy has not tried it. Dr Kennedy presents nothing in the way of proof of his proposition. He makes many statements bearing upon it, which different physicians will value differently, but he gives no positive proof.

Criticising Dr Kennedy's grand proposition, Dr Denham, Dr Atthill, and Dr M'Clintock (*Dublin Quarterly Journal of Medical Science*, August 1869, pp. 276, 262, and 232), show, that so far from the data lending it any support, they at least appear to be hostile to it. While Dr Kennedy gives no statistical evidence in proof of his proposition, they adduce abundant evidence to show that it is in the highest degree improbable. For instance, Dr Atthill produces the following:—

TABLE II.

| Years. | Deliveries. | Deaths. | Rates. |
|---|---|---|---|
| 1800 to 1804 inclusive, | 8,990 | 134 | 1 in 67 |
| 1805 to 1809 „ | 12,691 | 81 | 1 in 157 |
| 1864 to 1868 „ | 5,758 | 175 | 1 in 31 |

The mortality in these periods of the hospital's history is in a sort of inverse proportion to the deliveries. This table does not disprove Dr Kennedy's proposition. It renders it highly probable that Kennedy's proposition is opposed to truth. In favour of his own proposition, Dr Kennedy adduces no evidence nearly as strong as this of Dr Atthill's against it.

But the Dublin Hospital data can, without difficulty or strain, be made to yield direct and exact evidence on the influence of aggregation upon its inmates; evidence, as already said, of the highest value, from the unity and other characters of delivery, from the length of the hospital's existence, and the consequent greatness of its figures. It is easy to avoid a misleading bird's-eye view, or a dangerous selection of special strongly-marked years that may favour a preconceived notion. The whole data can easily be interrogated, and the answer taken for every degree of aggregation. That answer will settle the question, so far as these invaluable hospital statistics can settle it, and the result will be certain and irrefragable. To obtain the solution of this question, then, I have arranged and tabulated the data as under:—

TABLE III.—*Showing Rate of Mortality per Thousand amongst the Inmates of the Dublin Lying-in Hospital for the Years from* 1757 *to* 1868 *inclusive, arranged according to the Number of Hundreds of Inmates.*

| Women delivered. | Rate of Mortality per Thousand. | | | Number of Years or Observations. | Mean Age of Hospital. |
|---|---|---|---|---|---|
| | Lowest. | Average. | Highest. | | |
| Less than 100 . . . . | 18·1 | 18·1 | 18·1 | 1 | 1 |
| 400 and under 500 . . | 12·3 | 16·1 | 18·4 | 3 | 4 |
| 500 „ 600 . . | 7·2 | 13·5 | 20·4 | 5 | 6 |
| 600 „ 700 . . | 4·4 | 15·8 | 30·8 | 8 | 14 |
| 700 „ 800 . . | 5·7 | 6·3 | 6·9 | 2 | 17 |
| 800 „ 900 . . | 8·4 | 29·8 | 72·5 | 3 | 49 |
| 900 „ 1000 . . | 5·4 | 7·4 | 10·8 | 3 | 24 |
| 1000 „ 1100 . . | 5·8 | 25·0 | 38·2 | 6 | 78 |
| 1100 „ 1200 . . | 12·8 | 30·4 | 51·9 | 4 | 88 |
| 1200 „ 1300 . . | 6·2 | 13·7 | 26·1 | 3 | 55 |
| 1300 „ 1400 . . | 5·9 | 12·9 | 22·5 | 5 | 63 |
| 1400 „ 1500 . . | 15·7 | 19·1 | 24·8 | 4 | 64 |
| 1500 „ 1600 . . | 4·7 | 11·8 | 21·9 | 6 | 56 |
| 1600 „ 1700 . . | 4·9 | 9·7 | 15·6 | 5 | 51 |
| 1700 „ 1800 . . | 7·6 | 15·8 | 27·6 | 4 | 53 |
| 1800 „ 1900 . . | 13·1 | 17·4 | 19·9 | 3 | 84 |
| 1900 „ 2000 . . | 5·6 | 11·6 | 19·0 | 8 | 80 |
| 2000 „ 2100 . . | 6·8 | 13·9 | 21·7 | 6 | 81 |
| 2100 „ 2200 . . | 5·5 | 10·6 | 21.2 | 7 | 81 |
| 2200 „ 2300 . . | 5·2 | 5·3 | 5·4 | 3 | 66 |
| 2300 „ 2400 . . | — | — | — | — | — |
| 2400 „ 2500 . . | 8·2 | 20·9 | 33·2 | 5 | 62 |
| 2500 „ 2600 . . | 4·8 | 20·0 | 22·8 | 5 | 60 |
| 2600 „ 2700 . . | 4·1 | 4·5 | 4·9 | 2 | 59 |
| 2700 „ 2800 . . | 9·5 | 12·5 | 15·6 | 2 | 62 |
| 2800 „ 2900 . . | 7·3 | 11·0 | 15·1 | 4 | 61 |
| 2900 „ 3000 . . | — | — | — | — | — |
| 3000 „ 3100 . . | 5·5 | 5·5 | 5·5 | 1 | 59 |
| 3100 „ 3200 . . | 29·4 | 29·4 | 29·4 | 1 | 63 |
| 3200 „ 3300 . . | 5·5 | 5·5 | 5·5 | 1 | 60 |
| 3300 „ 3400 . . | — | — | — | — | — |
| 3400 „ 3500 . . | 9·2 | 9·2 | 9·2 | 1 | 61 |
| 3500 „ 3600 . . | 15·8 | 15·8 | 15.8 | 1 | 62 |

Any one can read off for himself the results of this table. It is well worthy of study; and to facilitate the observation of its general conclusions, I give another condensed view of its facts.

TABLE IV.—*Showing the Rate of Mortality per Thousand amongst the Inmates of the Dublin Lying-in Hospital, arranged according to the Numbers of Inmates.*

| Women delivered. | Rate of Mortality per Thousand. | | | Number of Years or Observations. | Mean Age of Hospital. |
|---|---|---|---|---|---|
| | Lowest. | Average. | Highest. | | |
| Less than 800 . . . . | 9·5 | 14·0 | 18·9 | 19 | 8 |
| 800 and under 1300 . . | 7·7 | 21·3 | 39·9 | 19 | 59 |
| 1300 „ 1800 . . | 7·8 | 13·9 | 22·5 | 24 | 57 |
| 1800 „ 2300 . . | 7·2 | 11·8 | 17·4 | 27 | 78 |
| 2300 „ 2800 . . | 6·6 | 14·5 | 19·1 | 14 | 61 |
| 2800 „ 3300 . . | 11·9 | 12·8 | 13·9 | 7 | 61 |
| 3300 and upwards, . . . | 12·5 | 12·5 | 12·5 | 2 | 61 |

Taking, then, the best data which, so far as I know, the world affords, as our sole, and as, in the meantime, a sufficient guide in this matter, we find that *the mortality of the Dublin Lying-in Hospital does not increase with the increased number of the inmates—does not rise with the aggregation. The mortality of the Dublin Lying-in Hospital is neither in the direct nor in the inverse ratio of the aggregation.*

The figures, indeed, seem to favour the view that the hospital is a better and safer institution the greater the aggregation. Certainly, a smaller proportional number die when there are many in it than when there are fewer. It is plain that we cannot look to aggregation as an important cause of mortality in the Dublin Hospital. This is a great practical result; for it sets inquiry into other directions to find out the hidden sources of increased mortality. Dr Kennedy's proposition, above quoted, is not only not proved by the data he refers to:—it is proved to be false. The opposite of his proposition is shown to be nearer the truth. With the fall of his Redan proposition, fall all his conclusions regarding puerperal fever and the advantages of small hospitals.

Although the supposed paramount evil influence of aggregation in this great hospital is now disproved, much more requires to be done with a view to discover and avert the causes of so-called metria, which is the chief source of variations in its death-rate.

# Part Second.

## REVIEWS.

*Haandbog i Therapien.* Af Dr Prof. OLUF LUNDT BANG, etc., etc. Anden omarbeidede Udgave. Kjöbenhaven: 1869. 8vo, pp. xi. 463.

*Manual of Therapeutics.* By Prof. OLUF LUNDT BANG, M.D., etc., etc. Second improved Edition. Copenhagen: 1869. 8vo, pp. xi. 463.

DR BANG ranks honourably among those hale veterans in medical practice, and in medical teaching and literature, whom it must always be a pleasure to the younger members of the profession to receive with respect and admiration. It has been his rare fortune to have been now for sixty years engaged in the practice of Medicine; and, already a lecturer on General Pathology in 1814, he has been a teacher of it also for a period but slightly shorter. During this unusually protracted career of labour, he has published, besides the Manual the second edition of which is now before us, several other meritorious works, including one or two on popular Medicine, with a variety of treatises in the Medical Journals. Among these works, we have for long had a more special acquaintance with his "Sygediætetik" (Dietetics for the Sick), published first more than thirty years ago; of which the hearty and indefatigable writer is now, we are glad to learn, preparing an eighth edition, all the previous ones having been exhausted, in fullest attestation of the public appreciation of its usefulness. Seldom, indeed, is it that the medical historian or critic has to record an example of mental and physical strength and endurance so remarkable as this: and that physician must truly have lived according to wise rules of his own art, who, after toils so lengthened, has not only not closed them, but still continues to pursue them with ardour and success.

A leading necessity, in the execution of such a work as that before us, has naturally been a desire to concentrate. Designed as a manual for actual practice, or as a text-book for academical lectures, it was fitting that, while in its nosological arrangement it should proceed, as it does, from the simple to the composite, and from the general to the special, its definitions of disease should be made chiefly to rest on those more patent characters that may be seen readily, and described easily. The details of pathology, and of pathological anatomy, properly confine themselves to still narrower limits, though they are nowhere undervalued. The proper uses of such a work, of course, presuppose, on the part of the competent reader, a due knowledge of the ætiology of diseases, and of the most approved rules and methods in semeiotics; though, in its more immediate scope, it admits only the laying down of the broader and more promptly accessible results, or of such as more especially fit themselves for the institution of a diagnosis as the bare groundwork for treatment. Without diverging towards pharmacodynamics, of which the treatise shows scarcely a trace, to his directions for the exhibition of medicinal remedies the author binds usually, with more or less of precision, those rules for diet and regimen which are so necessary for maintaining or modifying the efficiency of the more aggressive agencies. In his array of the latter, we recognise, as it were, the history of the experience of his long life passing in detail before us: each disease, in the successive order of its remedies, showing too often the fickleness with which the fashion of the day clasped hands with one or other of them for a while, and then coldly quitted it to greet some newer claimant for favour; leaving thinkers to stand by and wonder, how, if good, they had ever been abandoned, or, if worthless, ever adopted. Nor does our veteran writer, clinging, as might have been expected, with the customary tenacity of age to his early predilections, refuse their just relative place to even the most recent innovations in practice. Thus, under the head of Neuralgia, if we find recorded epispastics of all varieties, anodynes of all qualities, belladonna and veratria ointments, iodine, cyanide of potassium, chloroform, acupuncture, moxas, electro-magnetism, arsenic, valerian, monster doses of carbonate of iron, quinine, bromide of potassium, and not a few, besides, of remedies vaunted of old, we have also Richardson's aspersions of ether, subcutaneous injections of narcotics, freezing applications, and others of the newer devices; yet with the somewhat nullifying confession at the end, that it is by large doses of opiates, internally, that the pain is, after all, most surely encountered and assuaged.

One thing seems manifest enough in Dr Bang's treatise, and that is, that he has not been himself anxious to appear anywhere as an innovator in therapeutics, but has contented himself with the safer, and by no means barren, function of a careful and judicious eclectic. It would have gratified us, however, if he had proceeded here at least so far as to have made a more frequent and direct reference to what had been the results of his own immediate experience; prolonged, and, we presume, ample, as that experience has been. Thus, in the admirable Commentaries of our own Heberden, while we learn that they, like the work before us, are the fruit of a "magna consuetudo exercitatioque medendi, et longa annorum series," we are pleased also to encounter such passages as "Cum juvenis adhuc essem, et medicinam ex libris præcipue hausissem, abhorrebam ab opio paralyticis dando;" and pass on with all the more eagerness to the interesting context. Willingly and respectfully we meet, too, in the "Enchiridion Medicum," the "Vermächtniss einer funfzigjährigen Erfahrung" of Hufeland, another patriarch in medicine, some similarly practical remark, to which he has attached his own individual authority by such a phrase as a "wovon ich ein glückliches Beispiel weiss;" or some personal reminiscences, such as, to leave unnoticed many other examples, he introduces into his comments on the use of emetics in pneumonia. It is in this way that these veterans in our art succeed in worthily stamping the truth with each his own peculiar signet; and their treatises become thus invested with a degree of individual interest that throws generally an air of reality over their views and statements, and adds to a like extent to their authority. But of this kind of faith and reverence, the proper tribute to age and long experience, Professor Bang claims scarcely anything at our hands. Topic after topic passes under the eye of the reader, without the barest reference to a direct personal experience in treatment. With much information of a sound and clear quality, we have much here also that suggests rather the meagreness of a catalogue than the copiousness of a guide with its authoritative directions and warnings. Thus, the practitioner or student is too largely left to his own powers merely, to choose among a multitude of remedial plans, none of them certainly without authority, but few, if any, duly tested or attested by the writer's special verdict; and for the addition of that we should have felt grateful.

In corroboration of these remarks, we had intended, in supplement to what we have already said on the topic of neuralgia, to have glanced, by an almost random choice, through the details of treatment of hypochondriasis (justly and briefly characterized), as well as those of apoplexy, pneumonia, and pleurisy, as presented by the author; but this would lead to too great an extension of our notice. Another topic, however, on which we chanced to alight, was that of alcoholism, and on this we may still permit ourselves one or two observations. If memory supplied the hint, it certainly could affect us with no prejudice regarding Danish drunkenness, to recall the charge of Hamlet, or of Shakspeare through him, that other nations, when speaking of his countrymen, were wont to "clepe them drunkards, and with swinish phrase soil their addition." Nor, even while we could not help hilariously remembering the simple and kindly, but thorough and incorrigible, drunkard, depicted a century and a half ago with such superlatively humorous details by Holberg in "Jeppe paa Bierget," was it with any inclination to accept this delineation by the Danish Molière as a real type of the peasantry of his country. On the contrary, we believe that Denmark, with regard to the virtue of sobriety, holds, now as hitherto, no worse position than its other neighbours of the North, and we fear that we cannot assign to it a better. But Dr Bang enters into no statistics on this subject. Nor, indeed, could such statistics form any proper part of his scope or plan; though what he has said on the head of drunkenness, its results and its treatment, is well said, and as exhaustively as was consistent with the briefness necessarily enforced upon him. In his diagnosis of a fit of complete intoxication from apoplexy, it may be objected that he seems to confide chiefly in the odour of the spirits in the breath. But what if the apoplexy have occurred after whatsoever extent of indulgence in drinking, even if it have not supervened upon an absolute intoxication? In such a case, where the anamnesis failed to instruct otherwise, we should prefer to trust to the circumstance, that no state of coma from drunkenness, that we have witnessed, is ever so intense that it cannot be interrupted, to a certain degree, by appropriate measures. Thus, we recollect an instance of a man found motionless on a roadside, in whom we were unable to recognise any odour of spirits, but where a piece of twisted paper, softened at the end, and thrust into the fauces, produced some uneasy movements, and, at last, a muttered objurgation of "Vade ad inferos" (though in less classical language), which left us in no doubt as to the diagnosis.

Our author gives the computation, with reference to the fatality of delirium tremens, that of those affected with the disease in its simple forms, scarcely three per cent. die; while, in the complicated forms, the deaths are thirty per cent. Without avouching this precise proportion, we can, at least, testify broadly to the general truth involved in the statement, and would recommend its consideration to all indiscriminating, yet not the less ardent, promulgators of new and successful methods of treatment. We may add, that the fact that sudden death by syncope, as we ourselves can attest, is by no means an exceedingly rare termination in this affection, claims also some attention on the part of those who confide for its cure in heroic doses of foxglove.

Dr Bang is one of not a few writers, the perusal of whose works ought to render us desirous that the medical literature of Scandinavia should be better and more immediately known among English readers; who will be pleased to find, in the sensible lucubrations of their Northern brethren, that they, least of any, are of that class, still not extinct anywhere, who smother ideas under words, or mistake words for ideas.

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*Injuries and Diseases of the Knee-Joint, and their Treatment by Amputation and Excision contrasted.* The Jacksonian Prize Essay of the Royal College of Surgeons of England for 1865. By WILLIAM PAUL SWAIN, F.R.C.S., Surgeon to the Royal Albert Hospital, Devonport. Pp. 252. London: John Churchill & Sons: 1869.

EVEN those surgeons who studied with interest and advantage this essay on its gradual publication in the British Medical Journal for 1866, will be gratified by its republication in the present more compact and accessible form. New cases have been added to it, and the question of excision for gunshot injuries is more fully discussed.

It may be thought that already in our language sufficient monographs on this subject have been written, and that in a special manner the work of the late lamented Mr Price rendered this one unnecessary. This, however, is not the case; for even in the five years that have elapsed since the publication of Price's paper, some advances have been made in the pathology of the tissues involved in the diseases which render the operation necessary, certain improvements in treatment—specially in after-treatment—have been devised, and a very large number of new cases have been tabulated, giving more encouraging statistics than ever.

The following Table will show very distinctly the amount of new material on which Mr Swain founds many of his observations. It bears no pretensions to completeness; indeed Mr Swain's own cases are derived entirely from five hospitals,—Exeter, Plymouth, Devonport, St Bartholomew's, and King's College (for two years).

| | Cases. | Recoveries. | Deaths. | Amputations. | Recoveries. | Deaths. |
|---|---|---|---|---|---|---|
| Recorded in Mr Price's book to 1865, . . . | 316 | 240 | 76 | 39 | 30 | 9 |
| Collected by Dr MacCormac, . . . . . | 74 | 49 | 25 | 11 | 7 | 4 |
| Tabulated by Mr Swain, . . . . . | 82 | 67 | 15 | 4 | 4 | - |
| | 472 | 356 | 116 | 54 | 41 | 13 |

So far as this Table goes, we see a gradual but steady improvement in the results.

The first chapter, which gives a sketch of the anatomy of the healthy knee-joint, might have been omitted with advantage. It contains nothing new, and is quite unnecessary. The next two chapters, on the morbid condition of the knee-joint, are short and sensible enough, giving a very fair idea of the present views entertained by *British* surgeons on the subject. Mr Swain teaches, we believe correctly, that disease never commences in the articular cartilage, but, originating either in the synovial membrane or the osseous structures, is communicated to the cartilages. The fourth chapter gives a very brief account of the wounds, dislocations, fractures, etc., which may place the limb in a condition requiring either excision or amputation. Chapters V. and VI. detail the history and method of performing the operation. Nearly all surgeons are now agreed as to the incision—that the old-fashioned H-shaped incision is quite unnecessary, and that a horseshoe U, extending from the back part of one condyle, across the joint to the back part of the other condyle, gives both freedom of access and an excellent drain, while, in certain cases, a smaller wound can be made by carrying the incision simply straight across the joint. Mr Swain's remarks on the different possible complications of the operation, and the various ways in which they may be met, are very sensible and practical. He lays special stress on the importance of having the patient thoroughly under chloroform during the process of bringing the cut surfaces into exact opposition, lest the delicate cancelli of the bone be destroyed by harsh rubbing together.

We cannot agree so thoroughly with regard to the after-treatment. We differ from Mr Swain on two points:—

1. He prefers the splint known as Price's modification of the Macintyre to the simpler, cheaper, and more efficient method devised by Dr Watson.

2. He advises that the thigh should be bandaged firmly from above downwards, and the leg protected with pads of lint and pieces of plaster *before* the operation. This method, while it may have the advantage of saving the time during which the patient has to be kept under chloroform during the dressing of the limb, has the very great disadvantage of causing engorgement of the limb, and also of exposing the permanent bandages to the risk of becoming smeared with blood during the operation, and thus greatly increasing the risk of suppurative odour and discomfort during the after-treatment.

In Chapter VII. a list is given of the diseased conditions of the joint in which the operation is admissible. Three points are considered in order.

1. The extent and character of the disease; on this point we quote the following most excellent observations:—

> "No operative measures should ever be undertaken whilst disease is confined, for instance, to the synovial membrane. In cases even of gelatiniform degeneration, the surgeon would be most culpable if he resorted to extreme measures at so early a stage of the disease as that in which the synovial membrane was alone affected. Indeed, not only is it incurring a wanton risk in the performance of what may be a needless operation, but there seems to be a peculiar fatality attending upon excision for disease of the synovial membrane alone. I have extracted from Price's list no less than twenty-one unsuccessful cases in which the operation was undertaken for the removal of diseased synovial membrane only. And Mr Cadge of Norwich writes, 'It will be generally found useless to remove the ends of the bones, when the synovial membrane is the primary and chief seat of disease.' The reason of this is, I think, obvious. In the first place, we lay open a joint, the main tissues of which are unaffected by disease; and the shock to the general system is in proportion to the integrity of the joint. In the second place, we expose healthy bone tissue, with the cancelli open, and not condensed by disease, and the consequence is that the risk of purulent absorption or infiltration is all the greater."—Pp. 92, 93.
>
> "Cases of advanced disease of the cartilage are eminently adapted for excision of the joint, especially if the operation be performed before the constitutional symptoms have become seriously aggravated. The bones are not often much involved, and it is sufficient to remove merely the extreme articular surfaces, so that there is not much shortening; and, in young subjects, the saving a portion of the epiphysis provides for the future growth of the limb." —P. 105.

The condition of the bone which the surgeon likes best to see on making his section, and which gives the best hope, not only of a safe, but a speedy cure, is thus described:—

> "We find the cavities of the cancelli diminished, and their walls thickened, and the whole surface presents a reddish pink hue from the colour of the serum exuded."—P. 107.

Under the same head of extent and character of the disease, we have an account given of the operation in traumatic cases in civil practice and in military surgery. The cases are few; some of the civil ones pretty encouraging, the military ones very much the reverse; indeed, out of 41 cases recorded here, only four recovered. Excision for deformity is the last question under this head.

> "This proceeding is, I know, open to very grave objections; and although many surgeons have performed the operation, and restored to their patients useful limbs, there is, to my mind, this grave drawback to its general adoption, viz., that the life of the patient is endangered, without the actual necessity having arisen for its being so."—P. 127.

Of nineteen such cases recorded by Hodges, eight died, and one came to amputation.

Under the second head—the constitutional conditions admitting excision of the knee—we may sum up Mr Swain's opinion in the following sensible axiom:—

> "It seems so reasonable that it should be a *sine quâ non* for the patient to be free from any other exhaustive visceral disease . . . "—P. 136.

Under the third head, that of age, some interesting cases are given, showing the amount of comparative shortening of the limb that had been operated on, and yet proving that, even with eight inches of shortening, the limb was more sightly and more useful than an artificial one. "As a general rule," Mr Swain says, "excision of the knee should not be practised on children under the age of ten years;" and that "excision of the knee in very young children is inexpedient."—P. 149.

A very salutary caution is given on the subject of patience. Cases of excision of the knee require much care and time before a good result is obtained, and surgeons should not be in too great haste to amputate for sinuses even after many months. By constant patient *picking* at the disease, and in some cases even by a repetition of the operation of excision, a good result may at last be obtained.

The last chapter is on amputation of the thigh for disease of the knee-joint. As might be expected, in it our author shows how rarely this need now be performed. When necessary, he rightly recommends the operation devised by Mr Carden of Worcester, or the other on the same principle recommended by Professor Spence, insisting that in every case the amputation should be performed as low down as possible.

An appendix of cases closes the volume. It contains 104, mostly cases of excision of the knee; the rest of amputation, painful stump, etc.

We have thus fully analyzed this work, on account of the interest and practical importance of the subject of which it treats. It will be seen that, though the work of a partisan rather than of a judge, it discusses vexed questions fairly, and will be found a useful work of reference by surgeons anxious to know the present state of opinion in the south and west of England on the subject of excision. The work is illustrated by thirty-six exceedingly ugly woodcuts, mostly taken from photographs, which, we hope, were unsuccessful portraits.

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*A System of Surgery, Theoretical and Practical, in Treatises by various Authors.* Edited by T. HOLMES, M.A. Cantab., Surgeon and Lecturer on Surgery at St George's Hospital., Memb. Corresp. de la Société de Chirurgie de Paris. Second Edition, in five Volumes, with Illustrations. Vol. I., General Pathology, pp. 783. London: Longmans, Green, & Co.: 1870.

THE previous edition of this very valuable collection of papers on surgery having been fully noticed in these pages as the volumes appeared, it might seem hardly necessary to do more than merely announce the fact of a second edition being published, and congratulate publishers, authors, and editor on its being required so soon.

Some important changes, both in the form of the work and the contents of this first instalment, demand a brief notice.

In the first place, it is a decided improvement that the second edition should be in five rather than in four volumes; as the fourth of the first edition had become so thick and unwieldy, notwithstanding attempts at compression, and use of small type, as to be very cumbrous.

We are not so sure that the addition of the chromo-lithographs and lithographs is really an improvement. The chromo-lithographs are, as usual, too highly coloured, and not particularly life-like, and some subjects only are honoured with illustration, which gives a sense of incompleteness.

Of changes in the contents of this first volume, the first and most noticeable and important is the omission of the more strictly pathological portion of Mr Simon's beautiful paper on Inflammation. This omission is due to the very high conception this distinguished pathologist has of the importance of the later observations on the pathology of inflammation, and his most conscientious and praiseworthy wish that his papers should be the fruit of a thorough study of all recent work. His own words will bear quotation, and explain fully the reasons for his rather tantalizing delay:—

> "The process of inflammation, as regards the intimate nature of those circulatory and textural changes by which it is constituted, is, at the time of the issue of this second edition, matter of the utmost controversy; or perhaps I should rather say, all previous doctrines on the subject are just now in the very crisis of a reconsideration, of which the morrow cannot be foreseen.
>
> "In this state of the case, and regard being had to the practical intentions of the present work, it is thought, on the whole, better not to discuss in this place the questions which are now open; but rather to set aside for the present all deeper pathology of the subject, with the intention of devoting a separate

paper to it at or before the close of the work. And meanwhile, in discussing the symptoms and causes of inflammation, the least possible reference will be made to pathological theories concerning the process."—P. 3.

Doubtless this pathological paper will be a very difficult one to write, and a very valuable one for those who have not the time, or, perhaps, the linguistic power, to make themselves masters of the advances in the pathology of inflammation made by German pathologists, chiefly of the Berlin school, within the last ten years. Mr Simon promises that in it will be found a notice of the researches of Stricker on the contractile power of protoplasm possessed by the capillaries, and the peculiar penetrability (perhaps elective) which is associated with this power; also of the experiments of Moritz Traube on the ultimate fabric of cell-membranes, seen in the light of the late Mr Graham's researches on the laws of liquid diffusion.

In anticipation of this paper, we may simply notice, that Mr Simon seems to adopt and homologate the remarkable views of Professor Recklinghausen regarding the pus-cell, published in Virchow's Archiv for 1868.

> "That pus-cells possess the contractility of amœbæ; in virtue of which endowment they can undergo infinite changes of form; can (as with a kind of will) protrude and again retract processes or pseudopods of their common substance; can thus alternately intussusceive and discharge particles of finely-divided solid matter; and can slowly migrate from spot to spot in the body, certainly in the natural interspaces of texture, if not even actually making ways for themselves. It follows that previous microscopical descriptions of the pus-cell—such descriptions as that which is repeated above from my text of 1860—must now be read with the important qualification of the pus-cell as dead; as that in fresh-formed pus in its natural state, those globular pus-cells represent but one of many phases, and so far as they are present, express only an accidental repose of the function of amœboid motility."—Pp. 10, 11.

With such aims and with so much ground to go over, we cannot wonder at, though we may regret, the postponement of the pathological portion of this admirable essay.

Several new paragraphs have been added to the practical part of the essay. For example, a note on the value of refrigerating apparatus in the treatment of inflammation, and another on the method recommended by Campbell of New Orleans, Vanzetti of Padua, and others, of treating severe inflammation of a limb by compression or ligature of its nutrient artery.

The other papers of the first volume have undergone less alteration. It is enriched by the additions of Mr Croft's excellent account of hectic and traumatic fever and treatment of surgical cases after operation, which is now in its proper place, instead of being rather shelved, as before, in the fourth volume. It contains some very good thermometric observations.

Mr Poland's paper on Tetanus has been enlarged by the addition of a few observations on the pathological changes which have been found in the spinal cord, and illustrated by a lithograph portraying the morbid appearances observed by Lockhart Clarke and Dickinson.

The scientific continuity of the paper on Tumours is certainly improved by both innocent and malignant growths being conjoined in the new edition, and placed in the hands of Mr Moore, who re-edits, with considerable additions, Mr Paget's excellent account of innocent tumours, and adds an introduction, in which he shows the relation which innocent and malignant tumours stand to each other, and the great difficulty there is in drawing any absolute line of demarcation between them.

Among simple tumours not noticed in the first edition, are *myxomata*, part of the account of which we quote:—

> " Myxoma resembles the loose gelatinous tissue, named mucous, which composes the substance of the umbilical cord, and is found in adults in the moister and soft connective tissues, and the vitreous body of the eye. From such tissue the myxoma is derived, as the fatty tumour is from adipose tissue, and the fibrous tumour from fibre. Myxomas are sometimes remarkable for the looseness of their build, and for the freedom with which, on section, their substance will trickle away in thick gelatinous strings. In their simplest form they present no other character than this, as in the eye, or in the brain, where they spring from the neuroglia. Sometimes, however, they are mixed with other textural elements, and grow (as we have seen in the septum of the nostrils) into a gigantic tumour, in which bone and cartilage are irregularly mixed with mucous tissue, no denser or more capable of supporting its own weight than is the corresponding shining substance of the hydatid mole. They also form in the connective tissue of a thigh, or of the neck near the angle of the jaw, in that of the nerves, the subcutaneous tissue, or on the breast. They have also been met with in the jaw."—Pp. 525, 526.

Gliomata, another set of tumours, owing their name and much of their history to Virchow, are also alluded to.

In the new edition of Mr Poland's paper on Animal Poisons, we find an account of *verruca necrogenica* or anatomical tubercle, the peculiar foliated or warty growth on the hands so familiar to demonstrators and pathologists, and yet so rarely alluded to in systematic works. Mr Poland recommends for its treatment the use of nitric acid, or the acid nitrate of mercury. We have found the frequent application of glacial acetic acid quite sufficient.

The account of snake-bites is much fuller than in the first edition, chiefly by quotation from the researches of Dr Weir Mitchell on the pathological appearances and cause of death in such cases.

Mr Moore's paper on Wounds of Vessels includes a much fuller account than was given in the first edition of the various methods of suppressing hæmorrhage, including acupressure and torsion.

The volume ends with Mr Savory's paper on Collapse. The papers on Burns and Scalds, and on the general Pathology of Fractures and Dislocations, are postponed to a future volume.

In conclusion, we have great pleasure in stating our conviction, that this second edition is still an improvement on the first. As before, the papers are very unequal in value; but in most of them an attempt has been made to bring them up to the level of the present day, without any very undue increase in length, which is the great snare to be avoided by authors and editors of this age.

*Essays on Physiological Subjects.* By GILBERT W. CHILD, M.A., F.L.S., F.C.S., Lecturer on Botany at George's Hospital. Longmans: 1869.

THIS is a republication of some papers, bearing more or less upon physiology, which have already appeared in the "Westminster Review" and other periodicals. It is the production of a highly educated man of a thoughtful disposition, interested in the questions of the day, fond of new ideas, and who is yet capable of greater and more systematic efforts. Though we are very willing to give the widest extension to medical literature, to include within it everything bearing upon the healing art and accessory sciences, there are some subjects treated of in this book which must be passed by. In one sense, everything in the universe bearing upon human nature, physical and mental, has a bearing upon medicine; but such questions as the theory of evolution, and its relation to theology, though at present of great interest, are really beyond our scope. Dr Child has a well-reasoned defence of vivisections, which is scarcely required for the readers of this journal, who know well enough that without the experiments of Marshall Hall, Reid, Flourens, Magendie, Bernard, Brown-Sequard, and many others, the science of physiology would not have existed. The author, among other considerations, advances that the lower animals are immensely less sensitive to pain than human beings, and even that the degree of sensibility to pain in mankind "varies directly with the increase of civilization." He proves this by citing from Mr Palgrave's book on Arabia the case of a young Arab who endured with great fortitude the removal of a bullet lodged in the muscles of the forearm. Civilization renders a man, or at least some men, less accustomed to pain and misery, and thus less able to repress any outward display of suffering; but we do not understand how an uncivilized man should not feel an incision as much as a civilized one, and we have seen European soldiers endure the removal of bullets with a fortitude not to be surpassed. What degree of pain brutes endure is not so easily settled. In the lower organisms it is probably not very great; but, as far as our own observation goes, the higher classes of animals are very susceptible to pain.

Mr Child's essay on "Physiological Psychology" is an attempt to make some of the conclusions of physiology upon the function of the brain and nervous system "intelligible to persons whose chief interest is in psychological rather than in physiological science." We can only say in passing, without either endorsing or combating the materialism of the writer, that we have a deep distrust of knowledge gained in this easy and superficial manner. There is no royal road to science, and no popular one either. Those who wish to understand the physiology of the brain and nervous system, as far as they are now understood, must not be satisfied until they have studied physiology out and out, in all its connexions and antecedents.

The book contains two well-written essays on recent researches on the production of the lowest form of animal life, and on the production of organisms in close vessels, which have already appeared in the "British and Foreign Medico-Chirurgical Review," and in the Proceedings of the Royal Society.

Our readers will no doubt remember the keen controversy on this subject between MM. Pasteur and Pouchet, by which we confess to have been borne to and fro. Though the majority of scientific men took the side of Pasteur, Mr Child is inclined to believe that the experiments and arguments of Pouchet and his fellow-labourers have not been sufficiently appreciated in this country. M. Pasteur has in an elaborate series of ingenious experiments shown, that where all possible germs have been excluded, no organisms appear in closed vessels; but again M. Pouchet has, by a rival series of experiments, shown something very like the contrary; and where they agree to try the same experiments the results are different.

M. Pasteur's experiments do not always succeed in other hands. Using air admitted at great heights on Mont Jura, where he imagines germs should be absent, or at least not so common, M. Pasteur satisfied himself that in fifteen cases out of twenty no traces of animal life were found in his decoctions. M. Pouchet, with two friends, ascends to even a greater height on the Pyrenees, but finds organisms present in every one of his eight flasks. Mr Child details some experiments of his own made in company with Dr Beale. He used the precautions which M. Pasteur himself speaks of as exaggerated, yet he found bacteria to be produced exactly under the circumstances in which Pasteur asserts that they do not exist. He believes that M. Pasteur examines his substances with a power no higher than 350 diameters, whereas it is impossible to recognise them properly even at double that magnitude. This objection, if correct, vitiates the whole series of Pasteur's experiments.

Mr Child is disposed to admit either that the germs of bacteria are capable of resisting the boiling temperature in a fluid, or that they are spontaneously generated. If in despair of the question being settled by human ingenuity we are disposed to fall back upon the consideration that the heterogenists have been driven from the whole field of animal life to seek for a refuge for their hypothesis amongst creatures whose extreme minuteness renders their study very difficult, and whose propagation from ova may be assumed from experience of larger organisms since it cannot be settled by direct observation, Mr Child objects to this view that the undeniable fact of fissiparous division in the hydra and other lower organisms is in like manner inherently improbable, and opposed to experience drawn from the rest of the animal world but then at least we have ocular demonstration of the tru

There is some force in the remarks of M. Pouchet, originally made by Needham: You torture your decoctions with re-agents and artificial arrangements, until you have succeeded in bringing about within your closed vessels a state of things incompatible with the production, or perhaps with the existence, of animal life; and because no microzoa appear, you attribute this to all germs having been successfully excluded. On the other hand, you declare that the precautions which your adversaries have taken in *their* experiments were insufficient, for no better reason than that these experiments have been attended with results different from your own. For example, is it not at the outset highly improbable that an atmosphere passed through sulphuric acid and solution of potash should be fitted to produce or sustain any kind of animal life? M. Lemaire has shown "That the mere fact of an infusion being enclosed within a hermetically sealed vessel, even without any application of heat, is in itself sufficient to check the production of organisms, for that in such circumstances fermentation begins, but cannot continue. This certainly tends to show that other conditions besides the mere presence of germs are required for the development of organisms, and that such conditions are interfered with where infusions are hermetically sealed up."

On the other hand, Dr Wyman has stated that, although organisms appear in infusions boiled for a few minutes, "yet if the infusions were boiled for six hours no organisms ever appeared." The pervading diffusion M. Pasteur has given, not without some reasons, to the germs of microzoa in the air, and the part he has assigned to them in causing fermentation, has been the foundation of the new method of treating abscesses by carbolic acid; and it is possible we may get a new theory of fevers where parasitic organisms are to do the work formerly assigned to catalytic action.

The subject of marriages of consanguinity is handled in a sensible and thoroughgoing manner. Mr Child rejects the notion that there is anything in the mere nearness of blood-relationship of the parents to cause degeneracy in the offspring. Dr Boudin's statistics may leave a suspicion that congenital deafness is occasionally the result of such marriages. We commend those interested in this point to consider Mr Child's remarks on the relation between the assumed cause and the effect. He shows, by instances drawn both from the vegetable and animal kingdom, that breeding in and in is not attended with any evil consequences, the very finest breeds of horses, oxen, and sheep having been produced by consanguine connexions which could not possibly be made nearer, and which have been repeated through many generations.

## *The History of Four Cases of Chronic Inversion of the Uterus.* By T. GAILLARD THOMAS, M.D., etc.

DR THOMAS is well known in this country for his work on the Diseases of Women, which was reviewed in our pages not very long ago. Besides, he is the partner of the highly esteemed Dr Metcalfe, who was educated in Edinburgh. The pamphlet which he has now sent us is a very interesting one, illustrating as it does the replaceability of inverted uterus of long standing; indeed, of however long standing.

Dr Thomas's cases illustrate the well-known value of persevering efforts at replacement, success coming after several hours of work repeated day after day. To this principle Dr T. adds another, namely, the pulling down again the partially replaced uterus, with a view to immediately replacing it with increased vigour, "*reculer pour mieux sauter.*"

But the chief thing in the pamphlet is the history of a case in which quite a novel treatment was successfully used. The recovery is very wonderful; for, not only was the patient subjected to the operation we are just to describe, but in the manipulation for replacement the uterus was completely perforated by the operator's finger; and, after the operation was over, intraperitoneal accumulation of blood took place, the bleeding reaching the peritoneal sac through this finger-hole. The operation for replacement consisted in the usual manipulation, but as this alone had fourteen times completely failed, Dr T. opened the abdomen, as is done in ovariotomy, and dilated by an instrument introduced from above the uppermost part of the ring of inverted tissue. Whatever may be said of the plan, it is at least true that Dr Thomas was bold, and succeeded.

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## *Cases of Ovariotomy.* By Dr SVEN SKÖLDBERG, Stockholm.

DR SKÖLDBERG is a young surgeon who is not unknown in this country. He was, not long ago, a diligent student in several of our medical schools. Especially he sought acquaintance with the great operators, Keith and Wells. As soon as he has returned to Stockholm and begun practice, he has tried his hand at ovariotomy, helping to establish the operation among his Scandinavian brethren. Dr Sköldberg may well be proud of the success he has achieved. He has performed ovariotomy twenty-one times, and has had only four deaths among the cases in which he has completed the operation, removing the tumour. In one of his fatal cases the tumour was a malignant mass of the size of an adult head. In four cases he performed exploratory incision, and of these four, one died of peritonitis fifty hours after the exploration. We have no doubt we shall hear again of Dr Sköldberg.

*On Aphasia, or Loss of Speech, in Cerebral Disease.* By FREDERICK BATEMAN, M.D., M.R.C.P., Physician to the Norfolk and Norwich Hospital. Pp. 124. Bacon Lewis: 1869.

THIS most interesting essay has already appeared at intervals in the pages of our contemporary, the *Journal of Mental Science*, and it is now republished as a pamphlet, in deference to the opinion of those whom the author feels bound to respect. It is an advantage to science that Dr Bateman has in this instance taken the advice of his friends; for however we may differ in regard to his conclusions, there can be no difference of opinion as to this being the most concise and comprehensive monograph upon the subject in the English language. Its very curtness militates somewhat against its true utility, though it does not deprive it of more than a passing importance, as, if not a reply to certain views which have been propounded, at least as a loud call of Question?

Dr Bateman's conclusions are: "*1st*, That although something may be said in favour of each of the popular theories of the localization of speech, still, so many exceptions to each of them have been recorded, that they will none of them bear the test of a disinterested and impartial scrutiny. *2d*, That he by no means considers it proved that there is a cerebral centre for speech at all, and he would venture to suggest that speech, like the soul, may be something, the comprehension of which is beyond the limits of our finite minds." This we conceive to be going a little too far, and we think that a wider view of the subject, and a little less curtness in dealing with it, would have prevented our author from making any such rash statement. The comparison is not a happy one. The existence of a soul, in the opinion of many, is altogether an open question. Granting it, a soul must be either an entity having little but a theoretical connexion with ourselves, or it is ourselves; that is, our vital force, the only indestructible part of us incapable of being transformed by correlation into any of our physical, chemical, or mechanical forces. We need not argue the point here; whatever a soul may be, speech belongs to quite a different category. It is not necessary for existence, as from any point of view a soul must be; it is merely given us to maintain our mutual relations, and aid in our mutual advancement. Speech must have a central organ, because, wide as its relations are, they are not coextensive with our frame, yet it requires to be correlated with many other faculties; there must, therefore, be some central point where these correlations meet. To produce intelligent speech, memory and the power of vocalizing are required, for without them even a parrot could not speak. But that is not all, for many aphasics speak as perfectly as parrots. Intelligence, combination, and reflection are all additional faculties called into play in the production of such speech as shall convey to another the meaning of the speaker. Each or all of these faculties may, however, be affected by inhibition, or by disease, partial or general (as applied to the frame or only to the locality), influencing the separate organs of these faculties. Speech, therefore, may be affected by many different causes, some of them organic, others only functional. There must, we believe, be a true organ of speech, and yet it may unquestionably remain apparently unaffected in many cases of so-called aphasia. As yet, we are but on the threshold of such inquiries, and we welcome Dr Bateman's pamphlet as a valuable contribution towards their elucidation.

# Part Third.

## MEETINGS OF SOCIETIES.

### MEDICO-CHIRURGICAL SOCIETY OF EDINBURGH.

### SESSION XLIX.—MEETING II.

*Wednesday, 1st December* 1869.—Dr OMOND, *President*, in the Chair.

I. *Dr Watson* showed a cast taken recently from the foot of the boy in which he performed EXCISION OF THE ASTRAGULUS AND OS CALCIS, in July last. The cast showed how a dense mass of tissue occupied the gap resulting from the operation. The posterior portion of the arch of the foot being gone, of course the patient could not expect to retain the spring of the foot; but in spite of this, not only was walking easily effected, but the foot, as was seen, presented a most seemly appearance.

II. A specimen of a large OXALATE OF LIME CALCULUS removed from the bladder, by lithotomy, of a lad æt. 17. This operation was chosen in preference to lithotrity, because the urinary passage was small, the calculus large, and of the hardest description.

III. An UPPER JAW which he had recently excised on account of epithelial cancer invading its anterior and external wall. The orbital plate was not interfered with.

IV. THE LEFT SUBCLAVIAN AND AXILLARY ARTERY of a patient who was brought dead into the Infirmary, a cart-wheel having passed over the left side of the chest. The whole of the ribs and the clavicle on the left side were broken; the chest cavity was found half full of blood; the cellular tissue of the thorax and axilla were extensively infiltrated with coagulated blood. The opening in the subclavian produced by the first rib was readily recognised.

V. The parts removed in a case of EXCISION OF THE RIGHT KNEE-JOINT, performed yesterday on a little lad. The disease commenced in the synovial membrane, but had extended and invaded the cartilaginous textures. Suppuration was just commencing in the anterior part of the joint to the inner side of the patella. The operation was delayed for some time in consequence of the ankle-joint upon the same side having been originally affected as well as the knee; but as the disease of the ankle had completely disappeared, the only objection to the excision of the knee was removed. The extent of bone removed was carefully limited, so as to avoid interference with the epiphysal junction. The vessels having been secured, the parts were carefully stuffed with moistened lint, and temporarily bandaged on the back splint. At the end of twenty-four hours, the site of operation was exposed, the lint removed, the blood-clot cleared away, one vessel secured, and the parts carefully adjusted by means of the splint, rod, and plaster-of-Paris apparatus.

VI. *Dr Joseph Bell* showed AN EXOSTOSIS about the size of an orange, which he had removed from the back of the fibula of a strong young man, æt. 26. He had pain in the affected calf for two years, and occasional weakness, but had noticed the enlargement only for a month or two before operation. The tumour projected in the middle line of the calf, covered by the dural muscles. A single incision, about six inches, gave free access to the neck of the tumour; the tendon of the soleus was divided, the leg strongly bent, and the muscles relaxed. A deep groove was cut in the neck of the tumour by the saw, after which it was easily separated by strong bone pliers. Care was taken to remove every portion of the cartilage of incrustation from the broadened-out portion of fibula from which the tumour arose; little blood was lost, and the wound healed almost entirely by first intention.

VII The CERVICAL VERTEBRÆ and SPINAL CORD of a young man whose neck had been broken. The patient, a powerful man, æt. 21, was lying resting on a heap of sacks, when a friend in play threw a sack of grain, weighing about 12 stone, on his head. On its removal, the sufferer was found motionless and powerless, with the exception that he could slightly stir the fingers of his right hand. His head was bent slightly to the right side. He was sent into the Infirmary, where he died from exhaustion and pulmonary congestion in about twenty-four hours.

A careful examination of the spinal column showed that the fourth cervical vertebra was fractured across its base, but with little displacement; that the cord was compressed by a dislocation of the left lower articular facet of the fourth, forced off the left upper articular facet of the fifth, the latter being also slightly broken; the cord and its membranes remained entire.

Dr Bell remarked, that in a similar case he would be strongly inclined to put on a considerable degree of extending force by the pulleys, in the hope of possibly reducing the dislocation, as there seemed nothing in the nature of the fracture which need have caused a fatal result.

VIII. *Mr Annandale* showed two casts illustrating the successful cure of an aggravated case of TALIPES EQUINUS. The treatment consisted of division of the tendo Achillis, and the careful application of a simple mechanical apparatus for five months.

IX. A large MEDULLARY TUMOUR growing from the lower end of the femur, for the removal of which he had performed amputation at the hip-joint. In performing the amputation, Mr Annandale did not amputate immediately at the hip-joint, but removed the limb below the trochanters, and then, having secured the arteries, dissected out the head of the bone, with the hope that this proceeding would not be so great a shock as direct amputation at the hip.

X. An example of CONGENITAL MALFORMATION OF THE RECTUM, in which a well-formed anus existed, but the greater part of the rectum was obliterated.

XI. A FATTY TUMOUR of an elongated shape, which he had removed from a rare situation, namely, at the line of junction of the scrotum and groin. It had no connexion with the inguinal canal, and had given rise to much pain and uneasiness.

XII. *Dr Argyll Robertson* exhibited AN EYE that he had removed from a gentleman on account of deep suppurative inflammation, attended with agonizing pain. The patient attributed the commencement of the disease to exposure to severe cold during last winter on the hills of British Columbia. After being under treatment for several weeks at San Francisco, without improvement occurring in his sight, he determined to return home, with the view of getting further advice. When seen by Dr Robertson, his right eye presented the appearance of chronic general ophthalmitis, the lens was opaque, the iris bulged forwards. The eye was excessively sensitive to touch, and the tension was much diminished. There was no perception of light with it, and the other eye showed some symptoms of sympathetic irritation. The pain was constant and very severe, affecting the patient's general health, and preventing sleep. Since the enucleation, the patient has experienced great relief, being perfectly free from pain, and able to use the other eye freely. On examining the removed eye, the choroid and sclerotic were found much thickened and altered in structure close to the entrance of the optic nerve, no trace of true choroidal structure remaining at that part, a section presenting under the microscope a fibrous matrix infiltrated with pus and lymph corpuscles. The retina was detached from the choroid, and a few drops of pus occupied the intervening space.

XIII. *Dr Joseph Bell* then read his paper, entitled NOTES OF THREE CASES OF RECOVERY FROM ACUTE PYÆMIA, WITH REMARKS, which will be found at page 605.

*Dr Matthews Duncan* remarked, that had Dr Bell extended his observations so as to include obstetrical cases, he would have found the recoveries from acute pyæmia to be more numerous than he seemed to suppose. All enlightened obstetricians had now, when speaking exactly, discarded the use of the term *puerperal fever*—a term only productive of confusion, and leading to erroneous views; for it was now well known that there was no such thing as a specific fever of childbed, all cases coming under that category being but varied forms of pyæmia, septicæmia, etc. Much doubt, however, still hung over the true pathology of these diseases, and therefore also over their diagnosis and susceptibility of cure. He, however, believed himself justified in regarding a case as one of pyæmia, in which there had been rigors, perspirations, and scattered abscesses; and sometimes such recovered in obstetric practice. He put considerable faith in the employment of quinine, but perhaps more in the free use of iron, which was of so much benefit in allied disorders.

XIV. *Dr Douglas* read his paper, entitled ABSCESS OF LUNG, A CASE, WITH RECOVERY, occurring in a girl of fifteen. The symptoms and treatment were detailed with great care and minuteness, and the rarity of such cases commented on.

XV. *The President* then delivered his Valedictory Address, which consisted in a concise review of the business which had occupied the attention of the Society during his term of office, and concluded with short obituary notices of those members who had died during the same period. Amongst these were many names well known in literature and in science, and some who were loved and esteemed for themselves alone, apart from their distinguished position in the medical world. The list included Dr Macaulay, of London; Dr Robert Hamilton, Dr J. G. M. Burt, Dr Sellar, Dr Begbie, sen., and Mr Archibald Dickson.

The public business of the Society being now concluded, the private business was proceeded with, when Dr James Andrew, Mr G. R. Gilruth, L.R.C.P.E., and Dr William Craig, were admitted as ordinary members, and the following office-bearers were elected for the Session 1869-70:—

*President*—Professor Bennett.

*Vice-Presidents*—Prof. Sanders, Dr Thomas Keith, Dr Matthews Duncan.

*Councillors*—W. S. Carmichael, Esq., Dr William Gordon, Dr John Duncan, Dr Thomas R. Fraser, Dr J. D. Brakenridge, Dr Claud Muirhead, Dr A. G. Miller, Dr Angus Macdonald.

*Treasurer*—Dr George W. Balfour.

*Secretaries*—Dr Argyll Robertson, 40 Queen Street; Dr Grainger Stewart, 32 Queen Street.

# OBSTETRICAL SOCIETY OF EDINBURGH.

## SESSION XXIX.—MEETING I.

*10th November* 1869.—Dr SIDEY, and afterwards Dr BURN, in the Chair.

Thomas Sheriff, L.R.C.P. and S.E., was admitted as an Ordinary Fellow of the Society.

### I. NOTE OF A CASE OF RIGID OS TREATED BY HYDROCHLORATE OF APOMORPHIA. BY DR MILNE.

In June last I was called to attend Mrs S., Melville Terrace, in labour with her fourth child. On arrival, I found she had been suffering for three or four hours, and the pains were pretty strong. The os was about the size of a sixpence, and rather hard. The head was below the brim. Previous labours natural and easy. Two hours after, on making a second examination, I was rather astonished to find that, despite pains of a second stage character, little, if any, progress had been made. I introduced my finger within the os, and describing its circumference rather firmly, I found it intensely firm and resistant. I then placed the patient under the influence of chloroform, and commenced to dilate cautiously with the finger. Little good, however, was done by these means, for at the end of eight hours the os still remained about the same size. I then procured a little tartarated antimony, giving two grains in two hours. This, though producing vomiting and ample nausea, secured neither the softening nor the expansion of the os. As the pains were continuing rather strongly, I administered 25 minims of liq. morph. hydrochlor., and left for some time. On returning three hours afterwards, I found that matters were still much the same, the morphia neither having moderated the pains nor helped to reduce the induration of the os. Having brought with me a little hydrochlorate of apomorphia, I resolved on giving it a trial, being aware of its power as an emetic. A solution containing gr.¼ was given. In about ten minutes the patient vomited, and the pains were subdued a little, being less frequent and forcible. On examining again, to my delight the os had softened and expanded immensely, a single sweep of the finger serving to complete it. The pains redoubled their force, and, in a few minutes more, delivery was accomplished.

*Remarks.*—Cases of rigid and unyielding os are familiar to most obstetricians who have seen much practice, and there are few of us but have spent many weary hours over instances of this kind. I would, therefore, not have thought of reporting it but for the fact that I obtained such efficient aid from a new drug, and that probably we have got a novel uterine relaxant of considerable power. I say probably, because a single case can no more test a remedy—establish a rule—than a few green leaves can make a summer. It is true that the very rigid os relaxed in a twinkling almost after the apomorphia had time to act, as if the one were cause and the other effect; but then the other relaxants may have helped to pave the way, and the growing exhaustion of the patient may also have pioneered. Further trial is therefore requisite before we can laureate it much, and this I intend giving it as opportunity offers. If it should maintain its character as a uterine relaxant—the antipodes of ergot—and effectually throw open the door of the uterus in extreme cases, I need hardly say that we shall have obtained a drug of unspeakable value; for a fundus and body contracting powerfully, and an os refusing to expand, is about one of the most distressing complications in midwifery. It is a house divided against itself, a perilous antagonism, and has been fruitful of such deplorable results as inflammation, rupture of the uterus, convulsions, exhaustion, and death. For myself, there are many other complications that I would prefer before it. The medicines that I employed prior to the apomorphia are those commonly used, and in general prove effectual; the chloroform and antimony, with a little fingering, proving a good tripod on which to rest in these cases. Here they were ineffectual. I may state that I have long since abandoned belladonna, from a conviction, that however infallibly it may dilate the sphincter of the eye, it is next to useless here. I have never bled in such cases, believing that the remedy is worse than the disease,—that is to say, while you may secure in some cases the solution of the rigidity, you do it at a very great ransom; you enfeeble a patient already weak, and the subject of other causes and sources of debility. Incising the os is a line of treatment I have only once had occasion to adopt, viz., in a case of cancer; and although it is supported by great names, and warranted by considerable success, yet we have always the feeling that the incisions may go further than we desiderate, and that the patient may, in consequence, fare all the worse. Of course there will always cast up, now and then, cases beyond the power of medicine, and amenable only to the scissors or knife; but wherever medicaments can accomplish the work they ought to be employed because less beset with danger.

I do not know that we can hope for much benefit from the apomorphia in cases of undeveloped os and cervix, such as in some cases of placenta prævia; at all events it ought to have a trial. Being also a sedative, it may in some instances lay an arrestment on the pains, the presence of which, until the os be dilated, is a source of peril. If it fulfil the twofold function of opening up the os uteri, and laying for a time an embargo on the uterine contractions, it will solve a difficult problem in this complication, and confer inestimable good.

In reference to the mode of administration of this drug, I may remark that it is much more powerful when injected into the cellular tissue. Were the stomach in any case, therefore, rejecting most things, it could be given by means of Wood's syringe.

## II. REMOVAL OF A LARGE UTERINE FIBROUS POLYPUS. BY DR MATTHEWS DUNCAN.

(This paper appeared in last month's Journal, page 504.)

Before reading his paper, Dr Duncan exhibited a fibrous tumour which had been removed from the uterus by Dr Whiteford of Greenock.

*Dr Keiller* had seen a case of fibrous tumour with Dr Craig of Ratho, where, on examination, an enormous mass was felt through the os, and an attempt was made to assist nature in expelling it. The attempt to remove it, however, was unsuccessful, and the patient afterwards died from exhaustion. The tumour was embedded in the substance of the uterus, and, perhaps, if cutting had been resorted to in combination with traction, the result might have been successful. The specimen was preserved in his museum. He had removed one or two tumours by enucleation. In such cases he was of opinion that it would be advantageous to dilate the passages before operating; and this might be easily done by means of the india-rubber dilator. By fitting the tube of the ether spray apparatus to the bag, inflation and dilatation were readily effected.

*Dr Duncan* said that the great thing in all operations of the kind was patience and perseverance. The chief difficulty in his case arose from the bulk of the tumour, and he thought that Dr Keiller's plan might be serviceable. In virgins, much difficulty sometimes arises from the tightness of the vaginal orifice; and cases have occurred in which the perineum was torn into the rectum.

*Dr Thomson* said the tumour removed by Dr Duncan was the largest he had seen. He had found Dr Keiller's bag extremely useful in many cases, but he doubted whether it would dilate the orifice of the vagina.

*Dr Bell* did not understand how there should be more difficulty in dilating the vaginal orifice than the os.

## III. CASE OF HYPERTROPHY OF THE CERVIX UTERI, WITH AMPUTATION. BY JAMES YOUNG, M.D.

Mrs M., æt. 37, complained of great prolapsus of the uterus, causing her considerable pain and inconvenience. On examination, I found her suffering from an hypertrophied uterus, with an enormous cervix hanging down several inches between her thighs. The following were the measurements:—From os to fundus, 5½ inches; the cervix alone measured 3¾; to the fold of the vagina the circumference of the neck was 6½ inches. Patient states that she had, 16 years ago, two male children, and that thereafter, in consequence of lifting heavy clothes boilers, she felt that she had injured herself in some way or other. She has suffered very much from the prolapsus for five years, but the cervix uteri has protruded only for the last twelve months. Since May 1869, the extruded portion had become much larger, and the mucous membrane thereof like ord nary skin, and quite smooth in appearance. I tried every kind of pessary, but all failed to give the least relief. I therefore advised her to have a portion of the neck amputated. Accordingly, the operation was performed, with the kind assistance of Drs Keiller and Stevenson Smith, on Monday, 25th October. Several points of interest presented themselves, and the mention of them may not be altogether uninteresting to obstetricians. The patient was put under the influence of chloroform, and the ecraseur was applied about 2 inches from the os, and gradually tightened by a very slow movement; but as the indentation was being made, the chain of the ecraseur snapped, and we were compelled to suspend the operation until Dr Keiller kindly brought one of his own instruments, and another from Sir James Simpson. We applied Dr Keiller's beautiful instrument, but foreseeing a certain risk of its breaking, we removed it, and applied a third ecraseur of Sir James Simpson's, and with which we proceeded to complete the operation. Very slowly and gradually the chain became buried in the tough, dense substance of the cervix, and in about twenty minutes the chain was tightened to its full extent. At this stage Dr Keiller found it necessary to make several incisions with a bistoury before we could detach the amputated portion of the cervix. The chief point of interest which I would call attention to, is the extraordinary amount of pressure which was required to be put on the ecraseur before the operation was completed, and which may be imagined from the fact that the stalk of the instrument was completely bent.[1] Doubtless, this may be accounted for by the fact that the strain was all on one side of the body. I therefore much prefer the instrument with the lateral movement, where there is less chance of such an accident happening. We applied a little of the saturated sol. of the perchloride of iron and glycerine to the lacerated surface of the cervix, both before and after the removal of the ecraseur. There was little or no bleeding, and the patient made a most satisfactory and rapid recovery, being up on the eighth day. No treatment was adopted after the operation, beyond the use of tepid water by injection. On examination on the eighth day after the operation, I found the cervix uteri shrunk up, and very much reduced in size. The patient now (sixteenth day) feels an amount of comfort which she has been quite a stranger to for two or three years.

*Dr Keiller* said that this was about the toughest cervix he had ever met with. The instrument used in the first instance was one he had frequently operated with, and, indeed, he believed it was the first ecraseur ever used in Edinburgh. He had, some time ago, removed a cervix, but the result was not so successful as in Dr Young's case, and this he thought was probably owing to too much having been removed.

*Dr Duncan* said he had performed this operation very often, and latterly had used the ecraseur. The instrument sometimes disappoints. The first case he had used it in was a lupus of the vagina, but the body of the instrument bent so much that he had to complete the operation by cutting. In removing the cervix, there was, no doubt, a danger of opening into the recto-vaginal *cul de sac.*

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[1] This instrument was shown to the Society.

*Dr Milne* had operated in two cases of malignant disease, and in one the instrument gave way; he then used the bistoury to complete the incision. Considerable hæmorrhage followed, but it was easily arrested by the perchloride of iron.

*Dr Gordon* said that Sir James Simpson, in operating upon a tumour, had broken two ecraseurs, and it was not uncommon, he believed, to have bleeding after the use of the instrument.

*Dr Andrew* said he had seen the ecraseur used abroad, connected with a battery, with success, and no bleeding.

## MEDICO-PSYCHOLOGICAL ASSOCIATION.

A MEETING of the members of the Medico-Psychological Association resident in Scotland and the north of England was held on the 25th Nov., in the hall of the Royal College of Physicians, Queen Street, Edinburgh. The Chair was occupied by Professor Laycock, and there were also present—Professor Balfour; Drs Skae, Royal Edinburgh Asylum; F. Skae, Stirling District Asylum; Lowe, Balgreen; Howden, Royal Asylum, Montrose; Howden, Haddington District Asylum; Addison, Imbecile Asylum, Larbert; Grierson, Roxburgh District Asylum; Clouston, Cumberland County Asylum; Wood (Secretary), Newcastle; Fairless, Bothwell; Tuke, Fife District Asylum; Sanderson, Musselburgh; Jamieson, Royal Asylum, Aberdeen; Sibbald, Argyll District Asylum; J. Bruce Thomson, Perth; Smith, V.P.R.C.P.; Smith, Durham, etc.

The *Chairman* said that this meeting had been called in accordance with a circular issued by the Secretary, and he thought it was quite in accordance with the rules of the Association adopted in August 1865. To the best of his recollection, the fundamental rule was, that the Association was established for the purpose of promoting social intercourse among its members, and for the advancement of knowledge in insanity and its treatment. He had to congratulate their diligent Secretary that so many had met for that social intercourse. The rules provided in no degree for any other meetings than the annual meeting; but a year ago last August a proposition was adopted at a meeting in London, that there should be quarterly meetings of the Association held in London for the purpose of scientific discussion and social intercourse. But he thought they showed a more than usual amount of enthusiasm if they were to travel, as some of them would have to do, many hundreds of miles merely to hear a paper read in London. Th luxury of social intercourse with their friends in the South would be too costly; and it was a very natural conclusion that they should have something of the kind in the North, and in a locality more accessible to the members.

After some discussion, it was proposed by Dr Tuke, "That the next meeting be held in Glasgow on the last Wednesday of April," which proposal was adopted.

### THE CLINICAL TEACHING OF INSANITY.

The *Chairman* then brought under the notice of the meeting the propriety of memorializing the Managers of the Edinburgh Royal Infirmary, with a view to greater facilities being given than at present for the clinical teaching of insanity. He stated that at present there was no clinical instruction in mental diseases available to the medical student, except under circumstances such as to constitute a practical denial of it. Those who had attended the Edinburgh Medical School must be familiar with the two wards in the Infirmary, containing from ten to twelve beds, known as the d. t. wards, in which cases of delirium tremens were admitted and attended, and where cases of acute mania were brought in and treated, as it were, on the spur of the moment. These were the sort of cases which came under the care of the practitioner in the first instance, before being taken to the asylum; and instruction in these cases would be most valuable to the medical students. The appliances for the proper treatment of these classes of cases had hitherto been very defective, and not such as modern treatment demanded. He had from time to time given instruction in these wards, but he need hardly say that cases of ordinary insanity were inadmissible to them. There were, no doubt, plenty of asylums, but there was no stimulus for a student availing himself of that means of instruction, and devoting the time and labour to it, even if the means were presented to him gratuitously, and it was hardly reasonable to expect the officers of asylums to instruct gratuitously. It was quite certain, and proved by experience, that unless students were required by the Board of Examiners to attend this department, they would not do it. The case of mental disorders held a very peculiar position. They had hospitals for the treatment of consumption, but there were no such forms to be gone through as patients being taken before the Sheriff and getting certificates as there was with persons who were insane; and the bad effects of such delays were fully shown by an old friend of his, Dr Thurnam, now physician in the Wilts Counties Asylum. One of the obstructions to the proper treatment of insanity was the keeping of patients away from the asylum so long as to render the cases incurable; and the extent to which cases were thus rendered incurable was hardly to be credited unless proved by very careful statistics, as they were by his friend Dr Thurnam. He stated that in twenty cases of insanity, not complicated with other diseases, admitted during ten years, from 1798 to 1808, as many as nineteen recovered; and he went on to prove that if cases were treated within three months of the first attack, four-fifths would recover, but if twelve months elapsed, four-fifths were incurable, and so in proportion as the term was longer or shorter. Hence the importance to the public that these obstructions to their treatment of cases should be removed. He therefore thought the meeting would do well to commend to the Royal Infirmary the setting apart of wards for the treatment of cases which must be forced upon the attention of the private practitioner. Insanity was usually included in a course of medical lectures, but in the lectures he gave on the subject he found it utterly impossible to teach without bringing the students in contact with the insane, and illustrating cases to them, and when he did so he went to an asylum and saw, cursorily, the progress of mental disease. The various causes of these diseases ought to be made a subject for examination of students; and something more was required of them than answers to questions such as the following, which he found were put to their students in medicine:—

"How does Aristotle define virtue? State carefully the precise meaning of *μεσότης* and *ὁ φρόνιμος*, and the influence of these ethical conceptions upon the definition of virtue, and upon Aristotle's ethical theory in general?"

"How far were ancient philosophers justified in connecting the idea of moral depravity with their conception of matter?"

In concluding, the learned gentleman referred to the increase in the number of cases of insanity, and he thought the members of the Association would do service to the public, and greatly advance the interests of medical science, by adopting resolutions to the effect that provision should be made for clinical instruction in the class of cases received in the wards of the Infirmary to which he referred; that medical students should be required to attend hospitals for the insane, and that there should be an examination of each in that special department of science, to see whether he was fitted for the performance of his important duties.

After a lengthened discussion, in which Drs Lowe, Sibbald, Skae, and Professor Balfour engaged, the following motion was proposed by Dr Sibbald, seconded by Dr Skae, and unanimously agreed to:—"Resolved, that this meeting desire to express a strong opinion of the necessity of making clinical teaching of insanity imperative in every medical curriculum, and request the Secretary to send a copy of this resolution to the Medical Faculties and University Court of Scotland, the Boards of Examiners in Scotland, and the members of the General Medical Council."

Papers were subsequently read by Dr Tuke on the "Cottage System of Management of Lunatics as practised in Scotland, with Suggestions for Amelioration and Improvement," which will be found in the January number of the "Journal of Mental Science;" "On the Medical Treatment of Insanity," by Dr Clouston; "On the Hereditary Nature of Crime," by Dr J. Bruce Thomson; and "On the Death-rate of the Insane," by Dr Howden; on all of which subjects animated discussions took place.

The business having been brought to a close,

The *Chairman* said that the meeting had been highly favoured by the President and Council of the Royal College of Physicians, who had placed at their disposal the beautiful hall in which they had met. He would propose that the Secretary should communicate their cordial thanks to the President and Council of the College for their hospitality.

The proposal was cordially agreed to.

On the motion of *Dr Howden*, a vote of thanks was given to Professor Laycock for presiding; and the proceedings terminated.

In the evening, the members of the Association dined with Dr Skae, on the invitation of the Directors of the Royal Edinburgh Asylum for the Insane

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DUNDEE MEDICAL SOCIETY.—The second monthly meeting was held on Wednesday, 1st December 1869.—Dr Wm. L. Gibson in the chair. Dr T. J. Maclagan was unanimously elected a member of the Society. Dr A. James Duncan read an interesting paper of a case of "Cerebral Abscess," which had occurred to him in his practice, which will appear in an early number.

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# Part Fourth.

## PERISCOPE.

A TRUE PERISCOPE involves a look all round; it is not often in medicine, perhaps too seldom, that we look backwards, yet it is only thus that we can truly learn what progress we are making. The following letter (for which we have to thank an esteemed correspondent) is not only interesting in this respect, but also as a pleasing example of the critical acumen and caustic humour of the great Dr Gregory:—

Edinburgh, Tuesday, 4th December 1810.

My dear George * * * * ,—What is thought in London of the king? and of his doctors? You would stare at an expression of Dr Baillie—"there being no failure in the soundness of his mental faculties"—very strange, when he is so notoriously insane as to those very faculties. I think I understand Baillie; who is himself a very *sound-headed*, *sound-hearted*, upright man. I presume he meant that the king's derangement continued in the form of high, flighty, violent delirium (*sine febre*), not tending to fatuity nor stupidity; this last or common stage of insanity is almost or quite hopeless. In violent insanity there is always some little chance—very little, I presume, ætatis 73, and in one who had been mad before—of a recovery; that is, a partial or imperfect and temporary recovery. I do not know what English doctors, or Englishmen in general, think of madness. I never yet knew even one instance of a person who had once been fairly and *bona fide* mad, who ever afterwards became quite sound and settled. They are always more or less odd, capricious, unreasonable, obstinate, untractable, and subject to violent relapses, often from very slight causes: sometimes these relapses are almost regularly periodic, implying that much more depended on the predisposition or internal state of the body than on the external occasional causes applied. Sometimes they remain melancholy and moping, the train of thought preternaturally slow; nay, sometimes the mind so fixed on one subject that they cannot attend properly to any others, so as to judge rightly of them. Sometimes the train of thought is preternaturally quick and desultory; evidently not under the usual power of the person. This is strongly shown by their quick speaking and rambling from one subject to another, yet they are thought well, and are said to be cured. But after even one attack of the disease, the predisposition or tendency to it, far from being removed or destroyed—as happens with respect to smallpox, measles, hooping-cough, and perhaps some other diseases,—or lessened, as happens with respect to scarlet fever, and the common continued fever (or jail fever)—is remarkably increased; just as happens with respect to agues, inflammations, as of the eyes, throat, lungs, stomach, bowels, liver, skin (I mean the erysipelas), joints (in rheumatism and gout), still more remarkable in hæmorrhages, and closer still in spasms, epilepsy, asthma, hysteria, etc. One fit of insanity effectually stamps on a person an indelible character—worse than priesthood—and implying constant distrust and suspicion of the most painful kind. Do you think John Bull is or is not aware of this? Would the Lords of the Admiralty trust with the command of a sloop of war a man who they knew had once been mad? Would the East India Company trust one of their ships to such a man? Would a rich Peer or Commoner (not mad) employ such a man as his steward, attorney, lawyer, coachman, or even as his physician? I should think not. But perhaps they would trust such a crack-brained fellow as their *chaplain*, and even give him a living. Do you know Adam Smith's definition of a parson?—"A fellow who has served an apprentice to be a hypocrite; for whom everything is to be done; and who does nothing." I heard it only three days ago, from excellent authority.

As to the pretended curing of madness. In many cases it is an honest mistake, the malady very frequently going off (for a time) spontaneously, as gout, asthma, epilepsy, etc., do. In such cases the doctor and his drugs have the credit of the cure. Sometimes is really cured, cut short by remedies, cool spare diet (purging, bleeding, especially about the head), in persons who have gone mad with hard drinking, or even habitual very full living, implying too much blood in the body altogether, and too great a flow of it to the brain—perhaps producing some inflammation in the brain. Sometimes it is cured by proper moral management of a madman, just as the habitual indulgence of violent unruly passions might be in one not mad—by awe, restraint, coercion—vulgarly called a strait-waistcoat.

Very often it cannot be cured even for a time, forasmuch as it has been found on opening the heads of some maniacs (I mean after they were dead), that there were fixed organic diseases in the brain, hardness (and as it is said, unequal hardness of different parts) of the brain, tumours, hard or soft, sometimes perhaps scrofulous, in it, for which organic affections we have no remedy. And in many cases, in which, from the madness coming by paroxysms, and going off for weeks, or months, or years, but still returning, we may be sure that it does not proceed from any fixed organic affection; and, of course, must refer it to some irregular morbid ACTION of the living parts—analogous to dreaming—to derangement from drunkenness, or from opium—to delirium from fever; still that morbid action, or, at least, the very strong tendency to it, appears plainly in many persons to depend so much on the peculiar original constitution of the brain, often hereditary, that we can no more hope to change it than to change the same person's complexion, features, stature, or sex.

I think I told you when you were here of a Greek apophthegm on which I had stumbled last summer, and with which I was much delighted. Who is the best physician? He (replied Trophilus) who is able to distinguish what *can be done*, and what *cannot be done*. That fellow saw very far into the millstone, and as his countrymen preserved his *bon mot*, it is plain that some of them had understood it, and seen its truth and importance.—Yours ever,

J. GREGORY.

**ON THE ACTION OF ANÆSTHETICS.**—M. Claude Bernard, to whose admirable methodicity in performing scientific experiments, and sagacity in seeing their results, Medicine owes so much, has of late been engaged in determining the effects of morphia and of chloroform, and of their combination or successive administration. He finds that morphia puts to sleep, but it also produces a peculiar form of excitement; an animal narcotized by this alkaloid, requires but a minimum of chloroform to produce insensibility; and a subcutaneous injection of hydrochlorate of morphia rapidly induces insensibility in an animal being brought under the influence of chloroform. Bernard believes that the sensitive cellule (whatever that may be) requires for the production of anæsthesia the contact of blood mingled with a certain amount of chloroform, but the act of elimination constantly removing this substance, sensibility as constantly tends to return, and is only prevented from doing so by the blunting effect of the morphia which suffices to preserve the anæsthetic action of the small amount of chloroform actually in the blood. On the other hand, when chloroform prolongs and re-establishes the somniferous action of morphia, when it ought normally to cease, M. Bernard believes that this arises because animals under the influence of morphia are peculiarly sensitive to the action of chloroform—M. Bernard says, because of the excited condition of their reflex excitability. This is difficult to conceive; the fact nevertheless remains the same, and is of importance to all who employ anæsthetics.—*Gazette Hebdom.*, November 1869.

HYDRATE OF CHLORAL is the drug at present exciting most attention. As yet, we know nothing very definite in regard to its action. M. Bouchut says it is a sedative to both motor and sensitive nerves. He says that if the chloral is not crystallized and sufficiently purified to disengage, under the influence of potash, chloroform fumes without the liquid becoming coloured, it is impure, and may be dangerous. He says it ought not to be given to adults in doses larger than five grammes (77 grs.), and that to infants, not more than one or two grammes (15 to 30 grs.) ought to be administered. It may be given *per orem* or *per anum*, the latter mode being said to be more rapid in its action. It is dangerous to administer chloral hypodermically in man. Arterial tension is always augmented during the sleep induced by chloral. The urine passed after a chloral slumber is neutral, and does not reduce the copper of Fehling's solution. Twenty-four hours afterwards, however, the urine is more dense, and it reduces the copper of Fehling's solution like a temporary glycosuria. The action of chloral is precisely that of chloroform, but it is produced more slowly, and lasts much longer. In some it produces all the symptoms of drunkenness; in almost all slumber, rarely associated with hyperæsthesia, more commonly with a remarkable degree of anæsthesia. The degree of anæsthesia seems to depend upon the dose employed; two to five grammes (30 to 77 grs.) according to the age, producing such complete anæsthesia, as to permit of the extraction of teeth, or the painless application of the Vienna paste. Chloral is also the most prompt and efficacious remedy in those cases of intense chorea, where the constancy and severity of the spasmodic movements threatens the life of the patient.—*Gazette Hebdom.*, November 1869.

[In our hands, thirty grains have proved a safe and admirable hypnotic; a less dose has never sufficed, and only in one case have any unpleasant symptoms been developed; these were, insensibility, with coldness and rigidity of the extremeties—phenomena which occasionally occur in the chloroform narcosis. Pure chloral must be very cautiously administered in doses over thirty grains. It seems to have no influence, save a merely temporary one, over the spasmodic movements of chorea.]

# Part Fifth.

## MEDICAL NEWS.

**Relapsing Fever.**—The following letter, which speaks for itself, conclusively proves that no true relapsing fever has prevailed at Mid-Calder. It is singular how so strong a statement was permitted, on so slender a foundation, to appear in a document so important as the Registrar-General's Quarterly Report:—

*(To the Editor of the Edinburgh Medical Journal.)*

Calder Bank, Mid-Calder, 25th November 1869.

Sir,—The fever at Mid-Calder, noticed in Registrar-General's Report, was not the relapsing fever of nosologies.

Some of the cases had a long intermission,—say two days.

Two popular remarks regarding it may be given, viz., peculiar dull, tumid, prominent eyeballs, and remarkable weakness of legs and loins during convalescence.—I am, etc., Walter Watson.

We regret to have to record the death of Frederick Penny, F.R.S., Professor of Chemistry, Anderson University, Glasgow. For a short time his health had been rather feeble, although he had continued to lecture up to the 11th of November. No immediate danger was anticipated, but about nine o'clock on the evening of Monday, 22d November, while conversing cheerfully with his medical adviser, he was seized with a severe spasm of the heart, during the continuance of which he died. Professor Penny was born in 1817, so that he was in the 52d year of his age. He studied chemistry in the London Apothecaries' Hall, under Dr Henley; and such proficiency did he acquire that in 1839, at the early age of 22, he was chosen to fill the Chair of Chemistry in Anderson University, rendered vacant by the removal of Dr Gregory to Aberdeen. He arrived in Glasgow a perfect stranger, but, backed by introductions from the late Professor Graham, he soon made his way. He found the Anderson University at a very low ebb, especially in his own department; and it will be sufficient to mention, in order to show how successfully he devoted himself to the duties of his chair, and how highly his lectures were appreciated, that while in his first year he enrolled 87 pupils, during last session the number of his laboratory pupils was the greatest, save in one instance, attending in any laboratory in the kingdom; and the attendance on his popular class during the same session rose to 541. As a medical jurist Professor Penny had acquired a deservedly high reputation, and his name will ever remain associated with the singularly philosophical and beautiful physiological experiments for the detection of certain alkaloids when used as poisons, where purely chemical tests fail. All will remember how wonderfully his powers were displayed in this respect in the Pritchard case. A chemist's evidence, as a rule, is founded on processes too technical to be readily appreciated by a non-scientific jury, but Professor Penny's evidence was invariably given with such remarkable clearness, and such unshaken self-possession, as to render him a most invaluable witness. Professor Penny enjoyed probably the largest practice in Scotland as an analytic and consulting chemist, and his untimely death will be widely mourned on public and scientific, as well as for more purely personal, grounds.

WE regret also to have to record the death of another of the distinguished men of Glasgow, Dr John Macfarlane, late Professor of the Practice of Physic in the Glasgow University, who died at Helensburgh on the 7th of December. Dr Macfarlane was born towards the end of last century, and was the son of the Relief minister at Bridgeton, and the brother of the late Rev. Dr Macfarlane of Duddingston, who died a few years ago, while holding the office of Moderator of the Established Church of Scotland. At an early period of his life, Dr Macfarlane was one of the surgeons to the Royal Infirmary of Glasgow, and in 1832 published a volume of clinical reports, distinguished and esteemed for their signally practical character. He also contributed various important articles to our medical periodicals. In 1852 he was appointed successor to Dr William Thomson in the Chair of the Practice of Physic in the Glasgow University. This appointment he held till 1862, when he resigned it on account of failing health. Upon his retirement from public life, his professional brethren, under the presidency of Dr Lyon, presented him with a handsome service of silver-plate, as a testimony of their regard for himself personally, and also of their respect for the efficient manner in which he had discharged the duties of the very extensive consulting practice he had so long held. Since his retirement, Dr Macfarlane has continued to reside at Helensburgh, maintaining to the last his geniality of disposition, and his interest in the progress of that profession to which his life had been devoted.

JOHN LINTON, M.D., F.R.C.P. Ed., has been elected one of the Ordinary Physicians of the Royal Hospital for Sick Children, Edinburgh, *vice* Thomas Grainger Stewart, M.D., F.R.C.P. Ed., resigned; and Dr T. Kay Gray has been elected an Assistant Extra Physician, *vice* Linton.

AT a meeting of the Fellows of the Royal College of Surgeons, Edinburgh, held on Saturday, 4th December, Dr James Bell Pettigrew, Pathologist to the Royal Infirmary, was elected Curator of their Museum, in the room of Professor Sanders, resigned.

THE GREAT WHALE, recently cast ashore at Longniddry, has been delivered, by the Cæsarian section, of a calf, twenty feet long, now in possession of Professor Turner of the Edinburgh University, who is at present engaged in investigating various points of anatomical interest in regard to these little known and unwieldy subjects.

A FRUITFUL VINE.—A woman living on the property of Sir Watkins W. Wynn has presented her husband, a labourer, with five children at a birth. A few days ago they were all alive. The Queen has sent her £7. Twice she has had three at a birth, all of whom have lived. A Welsh correspondent tells us the poor woman has twenty-two children.—*Shrewsbury Paper.*

ROYAL COLLEGE OF PHYSICIANS OF EDINBURGH.—At the annual election meeting of the Royal College of Physicians of Edinburgh, on Thursday, 2d December, the following office-bearers were elected for the ensuing year:—

*President.*—Dr Andrew Halliday Douglas.
*Vice-President.*—Dr John Moir.
*Council.*—Drs John Moir, Robert Bowes Malcolm, Alex. Wood, Robert Paterson, James Matthews Duncan, Professor Sanders.
*Examiners.*—Drs Bell, Wood, Douglas, R. Paterson, Wright, Keiller, Pattison, Cumming, Duncan, Haldane, G. W. Balfour, Struthers, Ritchie, Stewart, Linton, Muirhead, Angus Macdonald, Fraser.
*Treasurer.*—Dr Samuel Somerville.
*Secretary.*—Dr Daniel Rutherford Haldane.
*Curator of Museum.*—Dr William Henry Lowe.
*Librarian.*—Dr James Warburton Begbie.
*Registrar of Applicants for License.*—Dr D. Rutherford Haldane.
*Clerk.*—Mr Christopher Douglas, W.S.
*Auditor.*—Mr Kenneth Mackenzie, C.A.
*Under Librarian.*—Mr John Small.
*Officer.*—Thomas Marshall.

ON the 14th of December, Mr Benjamin Bell, F.R.C.S.E., was presented by the male and female workers of the Blind Asylum, Edinburgh, with a very handsome gold watch, bearing a suitable inscription, as a small token of their loving gratitude for his great kindness and attention to them, for the long period of thirty years, during which he had acted as their medical attendant, at no little personal inconvenience, and for a merely nominal honorarium.

ON the 3d of December, Dr Alexander Russel Duguid of Kirkwall, Orkney, was presented by his friends and patients with a handsome testimonial, consisting of a silver claret-jug, bearing a suitable inscription, and a purse of sovereigns, on the occasion of his completing his fiftieth year of practice, and asa mark of their esteem for his professional and social qualifications.

# Part First.

## ORIGINAL COMMUNICATIONS.

### ARTICLE I.—*A few Remarks on District-Physician John Finsen's Contributions to our Knowledge of the Echinococci Endemic in Iceland.* By Dr JOHN HJALTELIN, Chief-Physician in Iceland.

IN the *British and Foreign Medico-Chirurgical Review*, No. lxxii., April 1868, an abbreviated translation of the aforesaid contributions may be found, mixed with some few remarks, most probably written by the translator himself. The first remark which seems necessary to be made on these contributions of Dr Finsen's is, that he seems very little acquainted with the best authorities on the helminthology of our time; for, while he only mentions a few, chiefly young and inexperienced, physicians in Copenhagen—as Christensen, Storch, Aarestrup, and others—he does not take much notice of the names of Dr Cobbold, Davain, Dr Charles Murchison, or Siebold, who, in our time, are the real authorities in this branch of the medical science. Eschricht, Rasmussen, Storch, Aarestrup, and Christensen have never been in Iceland; and of all Dr Finsen's authorities, only Dr Krabbe has been here for a very short time, out of which the most part was spent in making some feeding experiments on dogs, wherein he was assisted by Dr Finsen. A short time afterwards it was seen in some Danish writings, that these two doctors, in 1865, had made the discovery that echinococci of man were, by their feeding experiments, demonstrated to give origin to the tænia echinococcus of the dogs; and this was propounded in some Danish medical journals with no little self-importance, without any regard to the experiments of Drs Spencer Cobbold, Davain, or Siebold. The experiments of Krabbe and Finsen were afterwards promulgated in Germany through Drs Küchenmeister, Leuchart, and others. By some of these German doctors the experiments and pretended discoveries of Dr Krabbe, and perhaps Dr Finsen also, were mixed with some curious stories, which have afterwards been promulgated by high authorities, no doubt without their being aware that they thereby spread about a degrading untruth, reflecting upon a harmless and by no means stupid little nation—viz., the Icelanders. One of these stories, which is to be found in Dr Spencer Cobbold's little work, "Tape-worms and Thread-worms," second edition, London, 1867, page 57, runs thus:—"On the authority of Krabbe, it has been stated by Leuchart, that for every 100 inhabitants of Icelan there are 1100 head of horned cattle; and every peasant has, on an average, six dogs. In Denmark there are 180 cattle to every 100 of the people. There are several of the Icelandic doctors who not unfrequently have upwards of 100 patients (afflicted with this disease) under treatment at the same time; the total number of such cases in the island being estimated at 10,000. By far the greater number of those patients, however, are in the hands of quacks, whose influence is the greater, because there are in all Iceland but six legally-authorized medical practitioners, each of whom presides over a district of about 1500 square (English) miles, embracing a population of about 1000 individuals. The treatment of the quacks is exactly suited to keep up the epidemic; for, amongst their remedies, the urine and excrements of dogs play a conspicuous part." There can hardly be another story of equal length which can contain more falsehood than this statement, and I have been grieved that renowned and skilful medical men, like Dr Spencer Cobbold and Leuchart, have been induced to take up a story like this, which, from the beginning to the end, is altogether false and erroneous. I have the pleasure of personally knowing the excellent and learned Dr Spencer Cobbold, and should he happen to read these remarks, I request him to rest assured of my high esteem, thankfulness, and kindest regards towards him, for his kindness when I last year visited him in London. The authors of this very disgusting and degrading fable, reflecting upon all our little nation, are to be blamed; but the aforesaid Dr Cobbold and Dr Leuchart must be excused, for they could not even suspect that a medical brother would bring forward anything but facts and truth. Here in Iceland it is a well-known fact, that Dr Krabbe and Dr John Finsen, who is an Icelander himself, worked together at Akureyri, in the north part of our island, and it is therefore highly probable that they must both have agreed in their views regarding the cause of our endemic echinococcous disease.

In order to be able to estimate the utility of Dr John Finsen's "Contributions to the Knowledge of the Echinococcous Disorder in Iceland," it is quite necessary that the reader of these "Contributions" should be acquainted with his behaviour during his stay here as a district-physician for ten years; and though I feel rather unwilling to do this, I cannot help thinking it to be my duty to explain this for the sake of medical science, and of those who, by his "Contributions," might otherwise believe in his narrative. When the aforesaid Dr Finsen came here in 1856, he was a very young physician, who had just passed his examination at Copenhagen; he had then no previous experience of the endemic diseases of Iceland, but he seemed to have a good deal of self-confidence, which I feel sorry to say is sometimes the case with young students coming from the University of Copenhagen. When he began his practical career here he was, like the other district-physicians, bound by our laws to give me, as the chief-physician of this country, his yearly report on the predominating diseases in his district; but, to tell the truth, I was not a little astonished when, among the real diseases in his reports, I found a very long list of patients, with the most insignificant complaints, otalgia, odontalgia, rheumatic pains, globus hystericus, etc., etc. This was, according to my view, not the way to lay the foundation of any true medical topography of our island; and I cannot help thinking that all reasoning regarding the medical statistics of Iceland, built upon such a register, must have proved fallacious, and of little use to our medical statisticians. In seems to me that medical statistics, built upon the mere names of diseases, mingled with a large collection of the most insignificant complaints of daily occurrence, must be very fallacious, and can never be relied on as a true foundation for medical statistics. Of course, if a physician wishes to make a great *show* of his ability as a surgeon or medical practitioner, it is very convenient for him to enumerate every insignificant complaint, for the average rate of mortality will be thus assuredly far more favourable than in the case of those who in their case-books only record the more severe and really endemic or epidemic diseases, which may be dangerous to life, and which comprehend not trifling complaints of daily occurrence, but real diseases. The word "disease" is certainly a very conventional one; but in a medico-geographical point of view, I think that the more insignificant complaints, which are never dangerous, ought not to be taken into account in our reports, in order not to disturb and confuse our records of real diseases and their relative frequency. If in our case-books we were to write down all the more insignificant complaints, as toothache, earache, sore throat, etc., we should be obliged to record the names of all those we have happened to meet during the year; for there are certainly few people who for a whole year enjoy complete health without any complaint or uneasiness whatever.

When Dr Finsen, in his aforesaid "Contributions," informs us "of the frequency of the hydatid disease in Iceland, and finds out that the comparative frequency of hydatids in relation to other maladies is only 1 in 26 cases," then we must remark, that this is quite contrary both to the old and the more recent experience. The late chief-physician of Iceland, Dr Thorstensen, who for nearly thirty years was the physician-general of this country, found out that about 1 in 7 of the inhabitants were really affected by this complaint. According to my own experience, and from numerous autopsies, I am inclined to think that traces of echinococci may be found in nearly every fifth dead body amongst adult Icelanders, in one or another of the internal organs. For about thirty years since it was a general custom in this country to have all deaths registered according to the name of the fatal disease; and it was then, by comparing the fatal echinococcous disease with other mortal sicknesses, found out that nearly every fifth adult died of hydatids or its complications with other maladies. This was accepted by Dr Thorstensen, Dr Thorarensen, Skaptason, and others. When Dr Finsen came here he went his own way, regardless of his older and far more experienced colleagues. He had evidently a good deal of self-importance, and this was very soon felt by his older medical brethren. When he began his career here there were three very able physicians—viz., Dr Hjalmarson, Dr Thorarensen, and Dr Skaptason, who during many years had, not only in their official reports, but also in various foreign medical journals, given many practical hints in regard to the hydatid disease. Dr Thorarensen had by that time made his name known both in German and French medical journals for having cured hydatids by electricity, and had moreover cured a great number by puncture, in which operation he had a great experience; Dr Skaptason had collected very instructive cases, which seemed to prove that the echinococcous disease is a hereditary malady; and I myself had at the same time proposed the internal use of tinctura kamalæ, which afterwards was used with considerable success both by Dr Thorarensen and Dr Hjalmarson. The young and self-important Dr Finsen cared not for this, but went his own way, and writes now in his own "Contributions," "that it will probably have surprised many that the physicians in Iceland, who beyond all others must have the opportunity of acquiring rich experience in this direction, should have remained perfectly passive, and not have made the least exertion to elucidate a disease almost peculiar to their country." I must, in the name both of my dead and also of my still living colleagues, protest against this, and I am prepared to show that they have, in a practical point of view, done more than Dr Finsen himself.

In the year 1862, a well-known English physician, Dr Leared, physician to the Great Northern Hospital, and member of the Royal College of Physicians in London, came here. He then wrote a very popular and clear article on the causes and preventive measures of the echinococcous diseases in this country. He showed that, according to the views of the best helminthologists of our time, this disease had its origin from a kind of tænia (the tænia echinococcus) very common in the Icelandic dogs. And as the views of this my esteemed colleague were quite in accordance with the writings of the best helminthologists, I translated this article, and got it printed in some Icelandic newspapers. The best and surest method to prevent this endemic disease would, according to Dr Leared's views, be to cure the dogs of this tænia echinococcus; and, in order to do so, he advised to give them pulvis kamalæ, which is now looked upon as one of the best remedies for tapeworm; and from this plan I got the idea of using the tinctura kamalæ against the Icelandic hydatid disease in man. Dr Leared's plan seemed both clear and practical; but although he himself wrote about it to one of the leading physicians in Copenhagen, nothing came out of it; and even Dr Krabbe, who came here two years later, did nothing in that direction, but thought that the best preventive measure would be to kill a great number of the shepherds' dogs. This plan was sent to our Diet; but it was demonstrated that Dr Krabbe had very much exaggerated the number of the shepherds' dogs in Iceland. It was also proved, that even though Krabbe's plan came into execution, there would nevertheless remain many thousand dogs afflicted with tænia echinococcus, from whence the hydatid disease might be disseminated both to men and sheep. The shepherd's dog is a quite necessary domestic animal in Iceland, for without it, it would be impossible for the peasants to gather their sheep from our mountainous regions. Every peasant must therefore, according to the number of his sheep, have a certain number of dogs; and, being liable to various dangerous diseases, they are sometimes bought at a very high price. I myself supported the plan of curing the dogs, and would at the same time have all the hydatids of sheep and cattle burnt up or buried in the earth, when those domestic animals are slaughtered; but both propositions are still not enforced by Government. Dr Leared himself may best be able to tell how his propositions were looked upon in Copenhagen; and I should like if he would; for sure it is, they have as yet not been supported by Dr Krabbe or Dr Finsen.

So far as I can see, the whole medical statistics of the echinococcous disease in Iceland, as it is laid down in the writings of Dr Finsen, is very inaccurate, and in no way to be relied on. The same, or even worse, may be said regarding his calculations as to the seat of the disease in the various internal organs of the body. Dr Finsen says, that in his 255 cases, the origin of the echinococcus was, in relation to the several organs, found to be—in 176 in the liver; 3 in the kidneys; 2 in the spleen; 52 in the abdominal cavity (but where is not mentioned); 7 in the lungs; 7 in the head (where?); 1 in the nape of the neck; 2 in the supraspinous region; 1 in the subclavicular, and 2 in the axillary region; 1 in the arm; 1 in the mamma; and 1 in the thigh. All these statements are neither confirmed by autopsy nor by clear clinical observations or microscopical facts. Dr Finsen takes evidently a good deal of interest in the numeric method, but I think that such a method ought always to be built upon reliable observations and accurate physical signs, and not mere conjectures or incomplete clinical observations.

I cannot help thinking that many of Dr Finsen's echinococci tumours, especially those in the external parts of the body, have been nothing but serous cysts, and no echinococci at all. Serous and atheromatous cysts are very common in this country, and they are commonly to be found on the neck ("hydrocele of the neck"), near glands and glandular tissues, in the thyroid and in the mamma, as well as in the subcutaneous tissues; but although I have operated on many of them, I never could detect any traces of echinococci but in one, and even in that case, which happened a long while since, I was not quite sure of my diagnosis. When Dr Finsen, on the other hand, tells us that he, in 7 cases, or only in 2 per cent. of all his cases, met with echinococci in the lungs, he has most likely overlooked many of them in that organ; for sure it is, that both according to my experience, and, I may say, according to the experience of all my medical brethren in this country, the echinococci of the lungs are far more frequent than is assumed by Dr Finsen. In the sheep, where the echinococcous disease is so extremely common in this country, the echinococci are found nearly as often in the lungs as in the liver. I am very often consulted by people with chronic bronchitis or broncho-pneumonia, and also with hæmoptysis, who spit up great numbers of echinococci and their membranes. Echinococci in the spleen are certainly of very rare occurrence, and I have only once met with echinococci in the kidneys. In short, I look upon the whole statistics of Dr Finsen as nearly worthless, and in no way to be relied on.

Dr Finsen's symptomatology of the echinococcous disease is equally erroneous; he says, "that echinococci may be present in external organs even for *several decades*, without producing any symptoms whatever indicative of this disease." This statement, which I have also seen set forth by some Danish physicians, who, to tell the truth, have either very limited or no experience in this disease, is only true to a certain extent; for sure it is, that the hydatid or echinococcous disease very often, and even in most cases, has, in whatever internal organ it may originate, its constant symptoms, which often may be said to be premonitory, in so far as they may occur without any considerable or palpable tumour. These symptoms are indeed variable, according to the seat and the more or less considerable growth of the parasite, but their chief point is the "functio læsa" of the affected organ. This rule holds good, whether this disease is found in the brain, or the spine, or the organs of the chest, or in one or other of the abdominal organs.

Of course the symptoms are very different, according to the function of the afflicted organ, but they are very seldom, if ever, wanting, and may easily be detected by due clinical examination. When the centres of the nervous system are affected, the symptoms may be either cramps, or paralysis, or anæsthesia, with or without mental disease. If the echinococcus is situated in the lungs, we have cough, shortness of breath, chronic bronchitis, with dull percussion in the affected part of the lungs, hæmoptysis, and expectoration of the echinococci themselves. In the liver the echinococcus will often, even in its early growth, cause jaundice, various well-marked dyspeptic symptoms, and sometimes parenchymatous hepatitis. It is true that many of the morbid symptoms caused by hydatids in their earlier stages are very puzzling, as they may depend on chronic inflammation or morbid growths; but the aforesaid statement of Dr Finsen, "that they even for several decades may be present without producing any symptoms whatever," is altogether erroneous, and does not give much credit to his clinical talent. When Dr Finsen, to prove his statements in this respect, adduces the autopsy of the rector Arnesen (see page 21 in his "Contributions") he is greatly mistaken; for, as I happen to have known this man for many years, I can, from personal acquaintance, testify that he was very dyspeptic, and had many other symptoms of derangement of the functions of the liver.

Récamier's treatment of the hydatids, employed by Dr Finsen in this country, has for a series of years been well known amongst his Icelandic medical brethren, but not a single one has tried it excepting myself; and, to tell the truth, I very soon got enough of it. As far back as 1863, one could read in some of the Icelandic newspapers a long article on Récamier's treatment of hydatids, written by Dr Finsen. In this article it was stated by this physician that, out of 116 patients, he had operated on 32 after the method of Récamier; out of these 32, 19 were looked upon as cured, 5 had died, and in 8 cases there was no result at all, although some of them had been under his treatment not only for months, but some as long as half a year. At this time the governor of the north part of Iceland attacked Dr Finsen for having unnecessarily, and in a brutal manner, tormented his own clerk by operating on him after the method of Récamier without any result whatever; and as Dr Finsen, in this his very boasting article, had spoken of the puncture of hydatids not only as a useless operation, but as a dangerous one, he came into opposition with some of his other medical brethren, of whom one at that time had punctured about forty with the best result. As a chief-physician of this country I could not bear that the governor of the north part of our island should meddle in this matter, and I therefore wrote an article in the defence of Dr Finsen. The governor said nothing, but some of our other medical brethren murmured against the behaviour of Dr Finsen, finding that he had been unjust against the result of their puncturing method. Dr Finsen insisted upon his own experience, and maintained that nearly all who had been punctured were lost; this was really as unfounded as it was unjust to his other medical brethren. But although I myself found the behaviour of Dr Finsen too bad, I resolved to try the method of Récamier in some cases where I thought it most fit, but I feel sorry to be obliged to tell that I lost nearly one-third of all those operated on in this way. In two of these cases, where an autopsy was allowed, I found that a very extensive peritonitis was the cause of death; and although evident marks of the action of the caustic could be seen on the hydatid cyst itself, there was not the slightest trace of adhesion to the interior of the abdominal wall.

At this time, or a little later, when I had quite given up the method of Récamier, and had returned to my puncturing method of the hydatids, I happened to read Dr Charles Murchison's excellent article on "Hydatid Tumours of the Liver; their Diagnosis and Treatment," and as the views and experience of this excellent physician were quite in accordance with my experience, I wa strengthened in my preference for the puncture of the cyst in this disease, and have since that time operated on a great number of hydatids with the best result; and should Dr Charles Murchison happen to read these my short remarks, I request him to accept my best thanks, not only for the theoretical and practical information on continued fevers and typhus, but also for the information contained in the aforesaid article on hydatids and their surgical treatment.

As to Dr Finsen, I feel convinced that his renewed attempt to bring forward the method of Récamier will be of short duration, for, although some theoretical German doctors, as Professor Küchenmeister, have looked upon it as a very practicable mode of treatment, I feel pretty sure that contrary views will be sure to gain ground in England; and it seems to me that even the statistical statements of Dr Finsen himself are the best arguments against this method of operating. Dr Finsen himself says that, during his stay in Iceland, he has had 255 hydatid patients. Out of this number 48, or not wholly 1 in 6, recovered. It is not clear how many of the 48 operated on he has lost, but certain it is, from his own previously printed statements, that he, in 1863, out of 32 operated on, had lost 5, or every sixth, and 8 were then treated without any result at all.

It is well known that Dr Davain operated on 12 patients for hydatids according to the method of Récamier: out of this number he lost 6, or about 50 per cent. This bad result, compared with one nearly as bad by Dr Finsen, ought to make medical practitioners a little more cautious in practising this hazardous method of operating in the hydatid disease.

As I am now preparing a monographic pamphlet on the hydatid disease of Iceland, which is intended to show the practical experience both of my medical brethren and myself in regard to this disease, I feel no occasion to enter more deeply into this subject at this time.

## ARTICLE II.—*Case of Unclosed Urachus, with Umbilical Fistula.* By THOMAS ANNANDALE, F.R.S.E., Surgeon to the Royal Infirmary.

ON the 7th September 1869, Dr James Sidey was good enough to ask me to see a man, æt. 39, who since birth had suffered from an umbilical fistula, through which part of his urine flows. The patient was a big, strong, healthy man, having all parts of his body perfectly developed. When making water in the upright position about two-thirds come out in a full stream through the umbilical fistula, the other third passes out in the natural way by the urethra in a strong, but small, stream.

When the patient lies down the urine flows out involuntarily through the fistulous opening, more markedly so when he lies on his left side; much less flows when he lies on his right side.

No water passes involuntarily as long as he maintains the upright position.

He requires to pass water regularly about every two hours, and in doing this it is necessary for him to unloose all his clothes in front and bend forwards. His health is good; but he has on one or two occasions passed fine calculous matter with his urine, and, in telling me of this, he remarked, "It is a wise Providence who has afforded me this second channel for getting rid of my water, and any stones which may form."

A careful examination of the patient showed that the genital organs were normal and well developed, and that the walls of the abdomen and pelvis were perfect.

The umbilicus appeared a little flatter than usual, and presented in its centre an opening with depressed margins. The opening was large enough to admit the tip of the little finger, and a No. 12 catheter was easily introduced into it, and passed down into the bladder. The inner surface of the opening and its margins was smooth and healthy, and there was no trace of irritation or excoriation in the surrounding skin. A No. 6 catheter was readily passed along the urethra into the bladder, with the result of determining a perfectly normal but slightly contracted condition of the urethral canal. It was suggested to the patient that the fistula might be closed by an operation; but he would not submit to any interference, and returned home, being quite satisfied to know that there was no danger in his condition.

*Remarks.*—The urachus is sometimes found pervious for a short distance at its vesical extremity; less frequently it remains open through the greater part of its extent, as in a case referred to by Professor Turner,[1] in which an old woman, with a uterus bicornis unicollis, had a remarkable pyriform-shaped bladder, the apex of which was formed by a portion of the unobliterated urachus reaching for nearly three inches above the os pubis. Another congenital malformation has been described, and is figured in Förster's work. It consists of an elongated condition of the bladder, the apex of which is connected immediately with the abdominal walls, which are at this point deficient, and communicate with the cavity of the bladder by means of a large fistulous opening.

It is, however, a very rare condition for the whole canal to remain open as in the case related.

Littre,[2] in 1701, read before the Royal French Academy of Science an account of a girl, 12 years old, who always passed her water through an opening in the umbilicus; the neck of the bladder in this case was closed by a fleshy growth. The same author states that he met with a case in which a young man, 18 years of age, had the vesical end of his urachus dilated to an extent equal to the breadth of five fingers. A large stone obstructed the neck of this patient's bladder.

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[1] Edinburgh Medical Journal, February 1865.

[2] Mem. de l'Acad. des Sciences, 1701.

In 1787, Boyer[1] exhibited the bladder of a man, æt. 36, in which the urachus formed a canal one and a half inches in length, and contained twelve calculi the size of millet-seeds. The canal was not a vesical sac or prolongation of the mucous membrane.

In June 1850, Mr Thomas Paget communicated to the Medico-Chirurgical Society of London the interesting case of a man, æt. 40, who since birth had passed a portion of his urine through the umbilicus. The patient applied for advice on account of frequent and painful micturition, and these symptoms were found to depend on the presence of a calculus, which Mr Paget removed through the umbilical opening. The umbilical fistula in this case was complicated with the protrusion of a hernia in this region.

Mr Bryant[2] has reported the case of a boy, æt. 8, who had a fleshy growth like the glans penis springing from the umbilicus. In the centre of this growth there was a distinct canal through which urine passed.

In all the cases of this congenital deformity which I have quoted, there existed some complication either in the bladder or at the umbilicus, but my case is singular in being perfectly uncomplicated. Had the patient consented to an operation, I think there would have been little difficulty in closing the umbilical fistula, more especially as the urethral canal was healthy and unobstructed.

## ARTICLE III.—*Case of Cerebral Abscess, consequent on Caries of Temporal Bone.* By DR ANDREW JAMES DUNCAN, Dundee.

(*Read before the Dundee Medical Society.*)

HELEN LESLIE, æt. 22, millworker, residing in Doig's Entry, Overgate, presented herself on evening of 23d November, complaining of very severe pain in left ear and side of head, which had come on the day before. It seems that since childhood she had been troubled more or less with a discharge from the left ear, dating from the introduction of a small stone into the organ, which, however, had been subsequently removed. She had always been somewhat deaf on that side, though at times she heard almost quite well. The woman with whom she lodges stated that she was always childish in manner, and weak in intellect.

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[1] Cyclopædia of Anatomy and Physiology, vol. i.

[2] Surgical Diseases of Children.

Pulse was quiet; tongue dirty. A hypodermic injection of morphia into the arm was administered, and a powder of jalap, with a tonic (acid and bitter infusion), prescribed.

*28th November.*—Seen at own home. Learned that she had been much easier after the injection, and had continued tolerably well a couple of days, when the pain had returned as severe as before. It came on worse at night, and remitted in the forenoon. Pulse was quiet; bowels confined. Ordered a powder of calomel and jalap, and four of the following powders:—℞ Quin. sulph., gr. v.; pulv. opii., gr. i. M. Ft. pulv. Sig., One at 4 P.M., and a second at 9 P.M., each night.

*1st December.*—She has been seen daily; and as yesterday she was much easier, the pain being nearly gone, she was put on quinine and iron. To-day she continues better; is able to be up; so leave was taken, with orders to show herself. This was Tuesday.

*7th.*—Again seen at own home. Told that she had returned to her work, and had continued quite well and free of pain till evening of the 5th (Saturday), when she had indulged in a heavy supper. During the night the pain in head returned as severe as ever, and continued very bad, though slightly abated at time of visit on Monday forenoon. Pulse was about 70; tongue dirty; dry at tip. No desire for food. Ordered a powder of calomel and jalap.

*8th.*—Pain of head is now frontal, and not in left side; is considerably easier at visit, but it had been severe during the night. Tongue moist; pulse quiet; childish in her manner—apparently making no effort to bear the pain—constantly moaning and crying out. In the evening, at the request of the lodginghouse-keeper, who found her very troublesome, a line was signed for her admission into the Infirmary, to which she was accordingly removed next morning.

She was not again seen by me; and for the further notes of her case I am indebted to the hospital report, drawn up and kindly placed at my disposal by Dr White.

*9th December.*—State on admission:—Patient is extremely deaf, and very curious and singular in manner. She complains of very severe headache, sickness, and vomiting. Tongue moist; pulse 96; no rash; herpetic eruption on upper lip. No discharge from the left ear.

To have sinapism to stomach, and pulv. Seidlitz iij. statim.

*Vesp.*—Vomiting has ceased, but patient is exceedingly restless; will hardly stay in bed; has grown very confused; cannot now answer questions as she did in the morning; pulse small and feeble, about 108; head to be shaved, and cold cloths applied.

Wine, ℥vi. statim. Four leeches to temples.

*10th.*—Patient passed a very restless night, constantly attempting to get out of bed, shouting loudly. The parts bled well after application of the leeches. Pulse small and feeble, but canno' be counted, on account of patient's restlessness; is not altogether sensible, and is still very deaf; skin hot and dry; no rash; tongue very dry in centre; no more vomiting; bowels moved twice; stools very black.

*Vesp.*—Much in same condition. Constantly shouting, and very restless. Head is bent backwards, and seems fixed in that position; as, when attempts are made to bend it forward, she cries out with the pain. Pulse about 108, small, and feeble; sordes on teeth and gums; no rash on skin; left pupil a little more dilated than right.

11*th.*—Patient lies unconscious, with head bent back as before. The jaws open freely, and the power of deglutition does not seem affected. Other parts of the body are quite mobile; tongue dry; refuses all food and drink offered her. To have beef-tea enemata thrice daily. ℞ Ol. crotonis, gtt. ij. statim.

*Vesp.*—Only part of ol. crotonis was swallowed; no stool; patient much in same state as at morning visit. And in this state she continued till her death on the morning of the 12th.

*Post-mortem* examination was made on the 13th. On removing the calvaria, the left internal ear was found to be in a carious state, so that a probe, introduced by external meatus, passed right into the brain. On raising the brain, a considerable quantity of purulent matter squirted out from a small opening in the dura mater in that part lying over the petrous portion.

In this case, it seems doubtful what had induced the accession of the acute symptoms. Shortly before, the patient had walked from Forfar, a distance of fourteen miles, the weather being cold at the time; and it is just possible that the fatigue and exposure may have induced the subsequent symptoms. However this may be, the case affords a good instance of the most common proximate cause of abscess of the brain; about one-third of the cases recorded by Drs Gull and Sutton, in their article on "Cerebral Abscess," in "Reynolds's System of Medicine," being due to disease of the internal ear.

## ARTICLE IV.—*Chronic Rheumatism and its Counterfeits.* By J. MILNER FOTHERGILL, M.D., Senior Resident Medical Officer, Leeds Dispensary.

THE name rheumatism—or, as it is now more precisely termed, chronic rheumatism—has been used as a generic term for persistent pains, unaccompanied by constitutional disturbance, irrespective of their causation, and has covered an unknown amount of carelessness and ignorance; while, as a verbal placebo, it has often been used to justify a do-nothing treatment, sometimes very reprehensible. This is less true, however, of the present time than of the past; and now an apyrexial condition, accompanied by varying amount of pain, is found to be the result of many other causes as well as the poison of rheumatism. It may be well to state at once what that poison is, assuming that rheumatism, or what we call rheumatism, is dependent on an altered condition of the blood: in fact, is the external expression of a material in the blood, either morbid, or normal in excess. Lactic acid is this material in the belief of most observers, but not of all. One thing, however, seems universally admitted, namely, that it is an acid. Lactic acid is a product of assimilation, and is the normal result of the digestion of amylaceous or saccharine compounds, each molecule of grape-sugar ($C_{12} H_{12} O_{12}$), being broken up into two molecules of lactic acid ($2 C_6 H_6 O_6$); and it has been further stated that the oxidation of lactic acid is the true form of hydro-carbonaceous combustion. This goes on through the agency of the soda in the blood, and is imitated in the reduction of a salt of soda and a vegetable acid, of whatever kind, to a carbonate, in which form it is eliminated by the kidney. "A combustion of an acetate or tartrate, consuming oxygen and producing carbonic acid, is, in all probability, strictly analogous to a natural burning of lactate of soda, always going on in the blood for the maintainance of the animal heat."[1] This theory is ingenious, but will require further corroboration before it is universally accepted: it seems very probable at any rate. This, however, is certain, that lactic acid is a normal product of the body, and that rheumatism is usually supposed to be the result of its presence in excess. Lactic acid is found in sweat, and the natural emunctory of its excess is the skin. Excess of lactic acid in the blood may arise in two ways—one from excess of saccharine matter undergoing fermentative change, and overrunning the powers of excretion—or from normal production with defective elimination. Both forms are commonly seen; the first, however, more in connexion with acute, the latter with chronic, rheumatism. This *materies morbi* seems to have an affinity for the fibro-serous textures, and thus, in addition to its partiality for the joints, fixes on the fibro-serous textures of the heart; and from the changes thus produced by its action, do we have the great majority of our diseased and altered valves. It is not, however, confined to the fibro-serous textures, and may be muscular, neuralgic, periosteal, or sclerotic. Rheumatism, in its causation, has some curious relationships. It has a singular and most inexplicable connexion with the urethra. Acton says, "It would almost appear that rheumatism enters the system by the urethra. Rheumatism is produced by the passage of sounds and lithotriptic instruments as well as by the application of foreign matters. Urethral inflammation only is requisite to excite rheumatism in those who are hereditarily disposed to it. In gonorrhœal rheumatism, arrest of the discharge is often the readiest cure for it, though at other times it seems to follow the arrest of the discharge." Of this curious connexion no explanation as yet has been given. Another curious connexion exists between rheumatism and atmospheric changes: the joints of the rheumatic are often as certain indications of coming changes in the weather as barometer or aneroid. Some, in fact, only suffer from pain at these periods of change. Rheumatism is an ailment connected with families, and a predisposition may be handed down for generations. Rheumatism is often a troublesome inheritance, and, in those so hereditarily predisposed, slight causes are sufficient to excite a very persistent attack. It is commonly strong and otherwise healthy families with which rheumatism is connected. Mitchell of Philadelphia, and after him Day in England, has advanced the idea that acute rheumatism is, in its origin, cerebro-spinal, and allied to cerebro-spinal meningitis; this view seems untenable, as the one is rarely fatal, the other is commonly so. Rheumatism seems rather to be associated with the organic than the cerebro-spinal nervous system.

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[1] Headland's Action of Remedies, 4th edition, p. 135.

The symptoms of chronic rheumatism are such as might be expected from a blood-poison, and its manifestations are various. "The affection may show itself in the joints, giving rise to stiffness, a dull aching, and pain produced by motion, but without heat, obvious swelling, tenderness, febrile excitement or marked sweating; or it may implicate the muscles in various parts of the body, occasioning stiffness as well as pain when moved; or it attacks both joints and muscles, or is seated chiefly in the sheaths of the nerves, leading to what is called neuralgic rheumatism; of which, for instance, sciatica often affords a striking instance."—(Da Costa.) One property it has of making rapid metastases, of suddenly quitting one part to manifest itself in another; on other occasions it lingers round one part with great pertinacity. Whatever form it assumes, so far as my experience goes, rheumatism is always accompanied by the presence of an acid, both in the secretions and excretions; and of this condition the readiest evidence is furnished by the saliva. More readily accessible, and of not normally so decidedly acid reaction as the urine, I have been led to prefer it for testing purposes. Its normal reaction is faintly acid, but frequently it is neutral, and is always so during the alkaline tide of digestion of Roberts. If the saliva gives an acid reaction, as shown by the reddening of litmus-paper, at all decided, especially during digestion, the presence of an acid in excess may be at once inferred. As to whether that acid is lactic or uric, it cannot furnish evidence. When the reaction is not acid, the complaint is essentially neuralgic and not rheumatic, and a clue is at once furnishsd to the more exact treatment. Where there is decided acidity, relief is always afforded by alkalies. Pain is often the prayer of a nerve for healthy blood (Romberg); and, when so occasioned, is most readily and certainly treated by removal of that cause, where practicable. An acid condition of the system is often seen in growing children, and is often termed rheumatism; this is the "growing fever" of Lugol. In some children, and indeed adults, who suffer from a deficient power of cell-formation—*i.e.*, the strumous—the presence of an acid is usual; it is, however, generally uric acid, and is shown by calculus, gravel, nephria, acid sweats and secretions, injured teeth from the constant presence of an acid acting on the carbonate of lime, sour breath, etc.; and this explains the efficacy of potash in these conditions. Potash acts as a veritable hæmatic and blood restorative, by aiding in the elimination of the uric acid, and thus producing a more healthy blood. "Thus the uric acid which constitutes the urinary excretion of birds, whose habits are active and temperature high, and whose blood is highly oxygenated, cannot arise from insufficient supply of oxygen, but must depend on some peculiarity in the tissues; so also in man, uric acid may be the effect of defective nutrition or primary assimilation, and not merely of imperfect metamorphosis or oxidation. The clinical history of the so-called uric acid diathesis supports this view, and shows that the remedy, in many cases, must be sought, not in the promotion of oxidation, but in modification of the nutritive processes."—(Broadbent.) In this condition potash, by relieving the blood of its *materies morbi*, and thus improving its quality, allows of a more perfect tissue-nutrition in which the blood-glands share, and are thus enabled to perform a more perfect chylous elaboration. Potash is found in the muscles, soda in the blood; potash is more associated with tissue-metamorphosis, soda with respiratory change. Potash is the remedy in excess of uric acid, soda is just as useful with lactic acid. Urate of soda (chalkstones) is insoluble; urate of potash is a readily soluble salt. Pain, general or local, is the usual accompaniment of deficient or defective blood. Whenever the blood is altered from its normal condition we have pain; thus in syphilis, ague, lithiasis, rheumatism, anæmia, etc., we have local manifestations of a general condition. Why, however, a blood-poison should manifest itself in a nerve or group of nerves, as is the case in rheumatic and other neuralgiæ—why the general condition should make itself apparent in one locality, or inversely—how it happens that one portion of the organism alone affords manifestations of a general systemic blood-poison,—is explicable enough in the case of injury or excessive action; but it is not confined to these localities, and may show itself in a manner as yet inexplicable. This is deserving of attention, and, if explained, may afford a clue to a deeper insight into the *modus operandi* of morbid action in its early stages than as yet exists. We know, from experience, that the pain resulting from these causes—the external or outward visible sign—is relieved by mercury, iodide of potassium, quinine, potash, iron, arsenic, etc., according to the cause; and that in these conditions these drugs are, one and all of them, true hæmatics—*i.e.*, they restore the blood to its normal condition.

A peculiarity of rheumatism is that, when localized, it is frequently relieved by motion; the rheumatic whose first movements are productive of agony, will get along pretty comfortably after he has been moving some time. This is well known in horses the subjects of "jack spavin," which is really a rheumatic exostosis. Other rheumatics can work all day in comparative ease, but toss nearly all night instead of sleeping. This is often the case with the hands and arms of washerwomen; it may, however, be made a question whether this is always rheumatism or not; for it may be often a form of myalgia—but of that anon. When thus localized, it affects certain parts, and there it sticks; in time changes of structure go on, and the sufferer, in addition, becomes crippled. This change may occur variously—a bony exostosis may bind together two sections of a joint, and, though most usual in compound joints, may occur in the lumbar vertebræ;—it may lodge itself in the tendons, and produce contraction and deformity; or it may lodge in the sheaths of nerves till effusion takes place, and, by compression, produce partial paralysis, as is often seen in sciatica. In this last condition the loss of power, and with it loss of sensation, complicates the diagnosis with some pressure or lesion in the nerve high up nearer the trunk. There is a great tendency for rheumatism to lodge itself in parts that have been injured—as a dislocated joint, a bruise or crush, or to linger round a pleuritic adhesion; and in accidents occurring to the congenital rheumatic, the painful consequences are often rather to be dreaded, and if possible averted, than the injury itself. As a blood disease it may show itself anywhere; it may attach itself to one place and persistently remain; or it may flit about without a fixed residence, and may simulate many other disorders—and it may be simulated in its turn. With local affections it is often confounded, and too frequently persistent local pains, indicative of serious mischief, may be allowed to pass unheeded as rheumatism. This is unfortunately the case, sometimes, with ulceration of the articular cartilages, and with osteal and periosteal affections; and when any doubt exists, great care should be taken to watch the nocturnal exacerbations, the shiverings, etc. In Langenbeck's "Archives," No. ix., is related a case of periostitis and osteo-myelitis of the femur, which was mistaken for rheumatism. The pain in the right shoulder, symptomatic of hepatic disease, and especially of an abscess approaching the serous surface of the liver, and the sympathetic pain down the left arm in some cases of heart disease, are often carelessly looked over, and their significance unheeded, by classing them as rheumatic. Intercostal rheumatism has included pleurisy, pleurodynia, broken ribs, herpes, neuralgia, the peculiar pain generally of the left side found in women, and so strangely connected with menorrhagia and leucorrhœa; the pain in either side, which is intimately connected with debility and anæmia; and again is confounded with that condition of pain and soreness of the muscles developed by overwork, and attended with both muscular and cutaneous hyperæsthesia, designated by Inman "myalgia." This condition of the interosseous muscles is probably often the real condition of the washerwomen's hands mentioned above. This pain, from stretching the white fibrous tissue, usually unyielding, is most agonizing, and was empirically applied by our forefathers in the rack and other methods of torture. But beyond all other pains, localized or otherwise, spontaneous or feigned, for simulation and dissimulation, is, unquestionably, lumbago. To say nothing of its being confounded with abdominal aneurism, intra-vertebral cancer, disease of the spinal cord, renal congestion, abscess, renal calculus, nephralgia, lumbo-abdominal neuralgia, sprains, etc.; it is often assumed to be present when the patient is really suffering from "weak back"—*i.e.*, pain evoked in the muscular sense, and conveyed by the muscular nerves of sensation from the weight of the head and shoulders hanging on the erector spinæ. This state of over-tension is exquisitely painful, and is often quickly relieved by taking off that over-tension by the horizontal posture; it is intimately connected with growth,[1] and with hysteria from hyperæsthesia.[2] It is usually undistinguishable, locally, from congestion of the venous sinuses of the lumbar vertebræ, but the difference can sometimes be demonstrated by the treatment. For instance, the cure for lumbago, so fashionable a few years ago, by croton-oil, was really a rapid unloading of the bowels and relief to the congestion, and was not productive of any good in genuine rheumatism. On this subject, Copland states that true rheumatic lumbago is not connected with affections of the lower limbs, as cramps, numbness, pricking or lancinating pains, etc., which follow on lumbar-venous congestion. Rheumatic sciatica has included structural lesions, neuritis, neuralgia, specific and non-specific; while rheumatism in the neighbourhood of the anterior crural nerve is often really pain produced by the compression of the nerve by a load in the cæcum. Uterine rheumatism is often a misnomer for obstructive menorrhœa, and sometimes even for carcinoma. Rheumatic headache stands responsible for a deal of diagnostic carelessness, and has thus led to a great deal of direct and indirect mischief. Rheumatism, in fact, has been made a respectable cloak for everything that is painful, from orchitis to bruises.

Non-localized rheumatism is as liable to mistake nearly as its localized congener, and is confounded with general neuralgic hyperæsthesia, hysteria, alcoholism, tertiary syphilis, lithiasis from granular kidneys, locomotor ataxy, etc. From hysteria it may be distinguished by the general aspect of the case—hysteria occurring in young females, often of hyperæsthetic temperament, or of evident delicacy, who are rarely the subjects of true rheumatism. From neuralgia it cannot easily be discriminated always. In neuralgia, however, there is no acidity of the secretions, and some rational symptoms may assist to determine the question. Neuralgia is usually connected with the distribution of a nerve or group of nerves: there is too, according to Trousseau, tenderness over the spinal process where the nerve emerges from the spinal canal and brought out by pressure; while Valleix advanced the theory that there were special "tender spots" over the distribution of the nerve. An intermediate form of rheumatic neuralgia is not uncommon—a disorder in which there may be both a rheumatic and a neuralgic element, and where the acidity is very slight—in fact, a species of hybrid requiring a treatment as composite as itself. Professor Leudet, of Rouen, has described a variety of chronic alcoholism, in which the predominant symptom is pain, which may vary in intensity, be deep-seated or superficial, and accompanied by anæsthesia or acinesis, cramp, and increased reflex movements in the lower limbs. Mr Higginbotham, of Nottingham, has described a similar condition under the name of alcoholic rheumatism, which used at one time to puzzle him greatly, but which, in time, he got to connect with alcoholic excess; and which, in fact, depended on it, being irremediable except by abstinence, and which has entirely disappeared on the sufferers becoming total abstainers. This affection, though necessitating a peculiar treatment in accordance with its cause, is a form of genuine rheumatism, as it is intimately associated with the use of fermented alcoholic liquors, where an acid is easily generated, and does not seem a concomitant on the use of distilled spirits, which not readily undergo acetous fermentation. Mr Higginbotham says that, even where structural change has gone on, relief from suffering may be obtained by abstaining from the exciting cause.

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[1] Hilton on Rest and Pain.

[2] Laycock, Ed. Med. Jour., vols. li. and liii.

Tertiary syphilis often presents itself for treatment under the guise of rheumatism, and may be either simple chronic specific periostitis, or, from the cachexia, may be a form of true rheumatism —*i.e.*, that an undue formation or deficient excretion of lactic acid may result from the syphilitic cachexia. If a history be vouchsafed, a suitable and more or less specific treatment may be at once adopted with a good probability of success. But when a history is not forthcoming, then a resumé of the symptoms alone will enable us to judge of the probability of a specific origin. Tertiary syphilitic rheumatism, however, evinces a decided partiality for the periosteum and shafts of the long bones, and is very generally accompanied by nodes, especially in the anterior surface of the tibia, which are almost pathognomonic. There is often, too, a more marked cachexia than is found along with non-specific rheumatism. The clavicle, humerus, and forearms are favourite seats of this form of rheumatism. As well as its favourite seats and accompanying nodes, there are evidences of skin and throat affections, a mutilated iris, etc., which will assist in forming a correct diagnosis.

Other pains, not in any way rheumatic, are not rarely confounded with it, and this is commonly the case with the pains of locomotor ataxy. Of three patients thus afflicted, under care at present,[1] two complain of "rheumatic pains" most bitterly. In these two the pains preceded any marked indications of the disease—in fact, were regarded as rheumatic pains until the other symptoms developed themselves. The pains, however, are not those of rheumatism, and differ from its pains most markedly in the intense suddenness with which they come on. In one patient the pain comes on at times like an electric flash, bringing him to the ground at once; at other times the pains are fixed, often in the knees, and persist for hours. In the other two, the aching of the limbs preceded any other indication, and was as truly rheumatic as could be; in fact, the pains, perhaps, were rheumatic along with the ataxy. Now, however, the pains are evidently not rheumatic, and are closely allied to those manifestations of pain called "spinal irritation." In the third case, the disease was very well marked before any pain was complained of, and the pain has never been severe; but in place of paroxysms of pain there have been prolonged paroxysms of choreo-convulsive disorder of motility. These pains, in locomotor ataxy, come on in severe pangs—"stabbing, boring, shooting like lightning, flitting from one place to another in a very erratic manner, and recurring in paroxysms lasting from a few minutes to many hours." Their suddenness is their especial characteristic, and should always put the medical observer on his guard to look out for the other indications, as loss of tactile sensibility, etc. These pains may be accompanied by a feeling of coldness, thus closely simulating some forms of rheumatism. The importance of them lies in the prognosis, as the pains of locomotor ataxy are not to be relieved by art, and the disease on which they depend is of the most intractable character. "It is evident that, when the nerve-roots are severed by disintegration of the posterior columns, the peripheral ends can no longer transmit impressions to the cord; but their central ends may still be connected with the gray substance of the cord, and may, therefore, convey to it those irritating impressions that are caused by the process of disintegration."—(Lockhart Clarke.) All treatment applied to the peripheral distribution of the nerves is useless, and experience has demonstrated that the only conceivable treatment—viz., of sedatives to the receptive centres—is equally ineffective. The pathological explanation of the cause of the pains, and also of their intractableness, clearly explains the diminished sensibility and numbness in the limbs beyond the lesion. Trousseau regarded these pains as the most constant precursory phenomena of the disease. It is easily understood how the pains should precede the symptoms of loss of power or co-ordination or sensation, as it is induced by the disintegrating action, before it can have gone far enough in the destruction of the fibres to be manifested in the want of conducting power, as well as why it should persist afterwards. To illustrate it by fracture of the spine: violent pains are felt above the paralyzed and non-sensitive parts, due to the inflammatory condition of the cord immediately above the compression, and marking the border-land between perfect nervous conduction and total abolition of it. When, then, rheumatic pains of a sudden or instantaneous character are complained of, great pains should be taken to make a thorough examination for the other early symptoms of ataxy.

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[1] I am here speaking of my Westmoreland experience, when the paper was written.

Still more closely allied to chronic rheumatism in character, causation, and chronicity, are the pains accompanying granular or contracting kidney. This form of renal disease is essentially chronic, and carries with it a train of symptoms which as yet are not clearly understood, especially as regards the symptom chiefly under consideration here—viz., pain. It is not to be distinguished from rheumatism by any casual observation, and perhaps sometimes will escape the most careful scrutiny. Intimately associated with the retention of uric acid, thus causing an acid reaction of the secretions and excretions—a blood-poison, thus manifesting itself anywhere—its resemblance to chronic rheumatism is nearly complete. No absolute distinction can be positively drawn except from examination of the urine; but in this form of renal disease albumen is often absent for long periods together, and thus the readiest witness is wanting. The urine varies greatly, however, in quantity, and when great in quantity is usually of low specific gravity, and contains granular casts, which, however, are often few in number and not easily found. When found they settle the question as to the presence of renal disease. An absolute diagnosis is, however, not always attainable. We are then thrown back upon the rational symptoms, and the distinguishing characteristics may be found to run somewhat in the following direction:—Rheumatism is associated with the fibro-serous textures; in lithiasis the poison has more affinity for the true serous surfaces, and is often the cause of pleurisy and peritonitis. Lithiasis more affects the muscles, and rheumatism rather the large joints. Diarrhœa, vomiting, and other affections of the mucous membranes, as bronchitis, accompany lithiasis; and in these it differs from rheumatism. Lithiasis is accompanied by headache, especially of the vertex (persistent and recurring vertical headache is almost pathognomonic of lithiasis), or the pain may be frontal. There is often, too, a want of power to collect the ideas so as to interfere somewhat with the patient's pursuits, if of an intellectual kind. In lithiasis there is a wakefulness at night, not entirely due to the pain, while frequently there is cardiac depression from the blood-poison, closely allied to that caused by the biliary acids when circulating in excess, but not characterized by such severe palpitation as occurs in biliousness. In this it differs from rheumatism. There are, too, in the skin manifestations of lithiasis; and the various pricking eruptions, and that irritable condition of the skin intimately connected with recurring erysipelas or erythema in elderly people, are part of its results. These patients are readily acted on by cold, and exposure of the face to a cold wind almost sets it in a blaze; and the cold, so called, thus caught is accompanied by an inflammatory swelling of the face,—in fact, renal dropsy of a passing character. Dr Bell, in his Lectures, states the connexion between gout and irritable ulcer, and I have known an irritable ulcer that resisted treatment for years yield in a few weeks to water-dressing, when potash and colchicum were administered internally. Except under certain circumstances, it is allied to rheumatism again in its absence of constitutional disturbance, its persistency, and its connexion with atmospheric changes, especially of temperature—to which change the sufferer from chronic renal disease is most sensitive—and in the character of its pain. One patient, whom I saw in consultation, was regarded as rheumatic, such perfect sciatica and pain in the anterior crural nerve had he, though the medical gentleman attendant had watched the urine, the specific gravity of which was sometimes as low as 1·005. There was, too, a more profound cachexia than usually accompanies rheumatism; and the result of the case pointed clearly to the poison being rather uric than lactic acid. That these two conditions of lithiasis and chronic rheumatism are intimately related, and often curiously blended, is certain; and so commonly is uric acid found in chronic rheumatism (so-called)—see Copland's article on "Chronic Rheumatism,"—and so frequently may all the signs rational and physical of rheumatism go along with evidence of renal mischief, that it may be practically impossible always to separate them. It is very desirable, however, to attempt to do so, as the one is not incompatible with recovery, or, at any rate, a fair chance of a prolonged existence, while the other is like a person slumbering over a volcano. The marked cachexia so apt to be found along with renal disease points to its being a blood-poison of a more grave character than lactic acid. The one produces no change in the finer tissues of the eye, while lithiasis does so; these changes are revealed by the ophthalmoscope, and perhaps ultimately the absolute diagnosis will thus originate. Already the changes thus recognised may be considered to go a long way towards settling the question. The difference in the results produced by each poison on the heart is most interesting, and is not without diagnostic value. The poison of rheumatism, the consequence of altered nutrition, and intimately associated with the processes connected with tissue-nutrition, acts on the valvular apparatus by its affinity for the fibro-serous textures, puckering and disturbing the valves; whereas the blood-poison in renal mischief is related to retrograde tissue-metamorphosis, and by it is the muscular structure of the heart affected. The blood, when laden with renal derivatives, flows with difficulty, and leads to change in the driving power of the heart; hypertrophy occurring when the restorative capacity of the system is equal to such compensatory change, or a mixture of it and dilatation, or maybe simple dilatation, according as the system is more or less unequal to it. In the more grave cachexia of granular kidney, an impression haunts me that iron is more serviceable than in rheumatism. Certainly it is a great protection against th structural lesions that follow in its wake. Iron appears to have a peculiar power over blood-formation, and exercises a remarkable protecting power over both blood and tissues in chronic cachexiæ. The emunctories of lactic and uric acid are allied, and in each is this importance of keeping up the action of the skin most plainly visible. Pathology tells us that the sufferers from chronic renal disease are numerous. Dr Murchison estimated the proportion so affected, in the deaths at the Fever Hospital, at one-fourth of the whole; while Dr Dickenson found a still higher proportion in a series of deaths from accident brought into St George's. It behoves us, then, to look more carefully into the cases of chronic rheumatism, as there is ground for strong suspicion that many of our chronic rheumatics (so-called) are really sufferers from a very serious form of renal disease. Grounds there are for strong suspicion, and in many cases for absolute diagnosis; but, speaking generally, these cases are not sufficiently investigated, and it is desirable that more should be known about the rational symptoms of gouty kidney than is known at present. Lithiasis and gout may depend on one and the same poison, but the symptoms of lithiasis and typical gout are widely different. Gout is a disease of marked character—lithiasis is not so; and between the symptoms of lithiasis and chronic rheumatism mistakes are being made incessantly; and I candidly admit, that not only in many cases which were once considered rheumatic, a wider knowledge of the subject and more careful observation have demonstrated the renal origin of the ailment, and along with it in many cases an improvement in the treatment, as evidenced by amelioration of the symptoms; but also that I do not feel very confident about being always able even yet to discriminate between them. Rheumatic gout or arthritis is due to an intermediate poison, at least so supposed, which is intimately associated with debility, either constitutional or passing, but is not usually confounded with rheumatism; and it is accompanied by enlargement of the joints affected, and by other signs peculiar to itself.

The treatment of chronic rheumatism and its counterfeits must largely depend on the diagnosis. In the localized forms of chronic pain the diagnosis settles the disease, and with it the treatment. Local applications are of great utility in the relief of pain, but cannot be considered to exercise much control over the malady itself. The unity of origin of nerve-supply to the superficial and the more deeply-seated parts directly beneath (the law of Schroeder van der Kolk) admits of external anæsthetics, giving relief to pain more intimately connected with the deep-seated parts out of direct reach, the impression made on the superficial distribution being reflected on to the morbidly-acting distribution beneath. Lint wet with chloroform under a watch-glass, opium and belladonna plasters, are more or less efficacious. The application of a strong preparation of aconite and glycerine spread on lint is more certain. Aconite, morphia, and chloroform mixed, and used as an external application, has its supporters. Subcutaneous injection is perhaps the most general favourite now. Trousseau's plan of a small ammoniacal blister dressed with morphia is not sufficiently resorted to. Flannels wrung out of hot water, and sprinkled with tincture of opium or turpentine, are useful when large surfaces are to be covered. The addition of a solution of potash to the tincture of opium is desirable when the joints are affected, as being absorbed, and directly neutralizing the irritating acid. No form of external application or sedative is to be despised, and none too confidently relied upon. When there is effusion within the nerve-sheath, and, from its compression, numbness and lack of power, acupuncture, according to Fuller, is most useful; then strychnia or galvanism to restore the conducting power. Local applications have a uniformity of character not to be found in the systemic remedies. The general treatment of rheumatism opens up a strange field of ignorance, presumption, empiricism, futility, and sometimes of something worse—*i.e.*, the consciousness of futility without the conscience to confess it. The sufferers, worn out with pain, persisting for weeks or months, will try anything and everything that will only promise them relief.

The principles on which the general treatment is to be conducted are of two kinds,—to speak scientifically,—one to rouse up the eliminant organs to get rid of the poison, and the other to prevent as far as possible the re-creation of it; and in practice these two are best combined. The natural springs, sulphur, alkaline, or iron, or combined, are the general favourites with those who can afford them; and the handsome appearance of spas and watering-places is evidence of the faith in them being widespread. We must, however, remember that natural springs are merely solutions of soluble salts in water largely diluted, and in this is the advantage. This may be simulated artificially by adding a drink of water to the dose of medicine; and patients who cannot very well get to watering-places may often thus be brought under the influence of alkalies or iron, who could not be treated satisfactorily before. The natural waters bottled can now be readily procured for such as desire them. Baths of these medicated waters, combined with an increased temperature, are often of service. Turkish baths, hot air, vapour, and, in fact, baths of all kinds, are useful. The various drugs that have been employed are legion; they may, however, to some extent, be classified. There are diaphoretics in the front rank of eliminants, and of these opium, ipecacuanha, guaiacum, sarsaparilla, mezereon, and a host of others, are drawn from the vegetable kingdom; while of minerals, sulphur, antimony, mercury, arsenic, and iodine, are the chief; diuretics, as nitre, cremtartar, bicarbonate of potash, colchicum, juniper, squill, broom, and buchu, have all been resorted to; purgatives, as aloes, gamboge (also diuretic), potash and soda laxatives, magnesia, etc., have also been resorted to and relied upon. To fulfil the second indication tonics are to be most relied upon, as iron, arsenic, quinine, strychnine; and the ordinary vegetable tonics, as cascarilla, cusparia, gentian, etc. Iodide of potassium often fulfils both indications. Combinations of various kinds are to be resorted to according to the varying requirements of the patient. Dover's powder, or pil. al. comp., or Plummer's pill at night, and a mixture of pot. iodide and sarza, is popular, or a mixture of guaiacum and muriate of ammonia may be supposited in the cold form of rheumatism. Donovan's solution is useful when there is any suspicion of syphilis, or a mixture of Valangin's sol., hyd. bichlor., and muriate of iron. Alkalies are often needed pretty freely, and may be given with Fowler's solution or any light form of iron; from their destructive and debilitating action, they should always be guarded by iron or some tonic, and when the acidity is great are of the greatest service. The pot. bicarb. with pot. iod. in a vegetable infusion, is a good form. Cod-liver oil is often of the greatest value; and in rheumatic gout little can be done without it and iron. Potash with colchicum in infusion of cinchona is a useful combination. Colchicum and quinine in pill are great favourites in the treatment of muscular rheumatism, as wry-neck, etc. Crem-tartar (potus imperialis) and ferri pot. tart. form a most efficient tonic in cases of mild acidity. The syr. fer. quin. et strych. after food, and some potash before, is occasionally most useful. Where there is a decidedly neuralgic element, tonics, and most especially nervine tonics, are clearly indicated. In mixed cases, I have found a pill of guaiac., arsenic, and iron in pil. al. et myrrh, most efficacious; the guaiacum often being slightly cathartic itself. Cit. fer. et quin. with strychnia, form a nice tonic. Amm. carb., with or without pot. bicarb., and fer. am. cit., go well together. Even when there is evidently a syphilitic taint, it happens sometimes that the pain will yield to general tonics, and the nodes to pl. acet. and opium, that have resisted a more specific treatment. When there is a suspicion of lithiasis the treatment must be arranged accordingly. For instance, opium and mercury must be carefully withheld, and preparations of the solanaceæ or potass. bromide given instead. To show the susceptibility to mercury in cirrhotic kidney, I may adduce the following case:—An old woman, with confirmed jaundice and a liver reduced in bulk, got four grains of hyd. biniod. in twenty-four days. The urine was laden with bile, and in hunting for bile-stained objects, I came on myriads of granular casts. The discovery made me at once stop the mercury, but already there was decided though slight salivation. Still more serious consequences have followed the administration of opium and morphia in these conditions of the kidney. Some patients, however, take them with impunity, while others are most susceptible; it is well, therefore, to be most guarded in their use in renal disease. The advance of knowledge may shortly make clear where the variation lies. The amount of kidney affected has apparently something to do with it. Whenever patients are found with a susceptibility to opium and mercury, a suspicion of renal mischief is at once generated. Iron in this condition is our mainstay. Buchu is of service where we wish to make a permanent impression on the kidney. When the heart is affected, the addition of digitalis will be found to aid materially. When there is any œdema, a purgative of pulv. jal. co., or even better, gamboge in crem-tartar, is indicated. Uric acid forms, with potash, a highly soluble salt, and in these conditions potash is a true hæmatic as well as diuretic. Potash and iron are best combined, and form the basis of treatment in these cases. Bearing in mind the diminished power of the kidney to eliminate, animal food should be given sparingly, and eggs, milk, and a farinaceous diet supposited. Cod-liver oil and a judicious stimulation may even be necessary, and the relation of fats to neuralgiæ must not be lost sight of. Warm clothing, flannel to the skin, are imperative. Scudamore advocates the use of "fleecy hosiery." Sudden changes of temperature are the great source of danger, and the utmost care must be taken at these times; they are a source of great discomfort to the true rheumatic, but to the sufferer from renal disease they are imminently dangerous. In the pains of locomotor ataxy no relief can usually be afforded; the only plan, that of reducing the brain's susceptibility to the painful impressions produced by the disintegrating process, is not successful. This melancholy condition admits of no relief by art. Between the hopeless pains of locomotor ataxy, the dangerous and insidious condition of granular kidney, and the comparatively harmless and hopeful condition of genuine chronic rheumatism, we should strive our utmost to form a diagnosis, as on it hinges the widely different prognosis, and the equally varying treatment.

## ARTICLE V.—*Removal of an Embedded Uterine Fibroid.* By JAMES WHITEFORD, M.D., Greenock.

ON the 23d March last I was called to see J. F., an unmarried woman, aged 30, whom I found suffering from severe menorrhagia.

It appeared that hitherto she had always been quite regular, never having been more (nor ever much less) than a week unwell at one time, nor had the discharge ever been excessive until this occasion. She had begun her usual period on the 19th, and nothing unusual had occurred until the night before I saw her, when she was alarmed by a sudden and great profuseness of the discharge. Her mother described it to me as enormous, and her pale appearance indicated that she had lost a considerable quantity of blood. A cold-water cloth had been applied to the lower part of the abdomen, and this had moderated the flow of blood. Without examining the uterus, I directed this to be continued, and a dose of the tincture of ergot to be given every four hours. This soon controlled the bleeding, which quite stopped on the 25th, but returned for some hours slightly on the 29th March. Iron and digitalis were taken daily during the interval which followed, as the patient was much weakened, and the pulse was quick and small. On the 20th April she began to menstruate again, and on the 24th the ergot was again begun, and continued night and morning until she ceased on the 2d May. The loss on this occasion was not great—not much more, indeed, than she would formerly have experienced when she was in her ordinary health—but certainly greater than one might have expected in one so pale and reduced; and the result was that she was again much weakened. The iron and digitalis were resumed and taken during the interval, until the commencement of the next period, the 21st May. After continuing two days moderately, the discharge again became profuse, but was again controlled by the ergot. On the 27th, however, I found that the discharge had been excessive since the night before, notwithstanding frequent doses of ergot. I now examined, and at once discovered the cause of the bleeding, in the presence of a large tumour, which could be felt above the pubes, extending to about an inch above the level of the umbilicus. *Per vaginam*, the womb was felt to be much enlarged, having the cervix developed as at the close of pregnancy, with the os closed. A sound being passed showed the uterine cavity to be behind the tumour. A cold douche applied *per vaginam* sufficed to check the flow, and this, with the ergot, was repeated at intervals for three or four days; but the hæmorrhage was not entirely stopped until, by means of a gum-elastic catheter introduced through the os uteri, a solution of the perchloride of iron was applied, which proved effectual for the purpose.

The condition of the patient was now one of extreme prostration; the pulse was above 140, and scarcely to be felt, so that I resolved to attempt the removal of the cause of hæmorrhage so soon as the patient should recover strength sufficient to permit of the attempt. Iron and digitalis were resumed as before, but the patient was unable to leave her bed for three weeks. Fortunately, the June period was missed altogether, thus allowing her further opportunity for repairing the loss. The July period came on in due course, and continued for a week slightly excessive, the ergot with a mineral acid being used from the beginning of the flow. At the end of that time I again applied the perchloride of iron as before, and stopped it. The next day (15th July) I proceeded with the first step of the plan proposed by Dr Matthews Duncan for effecting this object. Using the scissors alone, I incised the cervix at either side, and having applied the perchloride of iron solution as before, I introduced a large sponge-tent. The following morning, on removing the sponge, the tumour was found presenting at the dilated os, the surface being smooth and tense. Moreover, the attachment of the tumour to the anterior uterine wall could now be distinctly made out, and, judging from the smooth continuous outline felt in passing the point of the finger from the surface of the anterior lip of the uterus on to that of the tumour, enucleation had not yet begun to take place. The manipulation necessary to ascertain this again started the hæmorrhage, but without further delay I incised the presenting part of the tumour with a bistoury guarded with lint (without the aid of the speculum), making a cut fully two inches long from behind forwards, and from a third to half an inch deep. To ensure against further bleeding, the perchloride of iron solution was immediately applied as before, with success. The next day I examined the wound freely, and found it still gaping. No hæmorrhage followed this examination. The August period was normal in duration and in the quantity of the discharge, although ergot was not used. The September period also passed over without undue loss, and a few days after its cessation I examined the parts for the first time since the incision of the tumour in July. A constant watery discharge had existed for some days, and the tumour was found bulging through the now fully dilated os, and presented a rougher or more uneven surface than formerly. The October period was rather excessive in the amount lost, but the character of the discharge was more serous than formerly. Ergot, with diluted nitric acid, in full doses, was now given thrice daily, and continued after the regular discharge had ceased. On the 19th October, a week after the termination of the regular period, a considerable hæmorrhage took place, but the injection of the solution of perchloride of iron again proved sufficient to arrest it. This loss of blood, however, had so reduced the patient that the slightest attempt to raise her head from the pillow was attended by signs of syncope, and to this was added constant nausea, which seriously interfered with her nutrition, so that her condition, in my opinion, demanded an immediate attempt to remove the tumour. This had now descended so far as completely to block the upper part of the vagina, so that the cavity of the uterus could be reached by the fingers only with difficulty. As I anticipated some difficulty from the narrowness of the *ostium vaginæ*, I endeavoured to dilate it somewhat beforehand, and this I effected partly by placing an empty bladder within the vagina, and then inflating it as much as possible, leaving it in this position for some hours. I now applied separately the limbs of a strong volsella (as figured at page 716 of the "Edinburgh Medical Journal," February 1867) as far up as possible, so as to ensure a firm hold of the tumour, and lower down another similar instrument; securing the handles of each with a piece of tape, steady pulling was now begun with the upper one, and when I stopped for a minute the lower instrument was held firmly by the nurse (who was my only assistant), so as to prevent retraction. With the fingers of my left hand I watched the claws of the instrument, so that when signs of tearing were felt, I reapplied it higher up. After two hours of steady pulling in this manner, assisted by the finger occasionally in separating a few bands of connecting tissue near the os, the tumour was successfully removed. Not more than a few teaspoonfuls of blood were lost, and only a slight discharge, serous mostly in character, continued for about a week after, and not much more in amount than had existed for some weeks before the operation. The nausea continued more or less until the next day, but neither pain nor abdominal tenderness; indeed, from first to last of the different steps comprising the operation, not the slightest symptom of metritis followed. Her recovery has been uninterrupted.

The tumour removed is perfectly bare of coverings, and measures six inches in length by four inches in breadth.

Since recovering from the operation, the woman's health has proved completely restored.

## ARTICLE VI.—*A Case of Aphasia.* By J. FAYRER, M.D., C.S.I., Professor of Surgery in the Medical College of Bengal.

THE following notes were taken at the time of a very interesting case of aphasia that recently came under my care. The patient was an English officer, holding an important post; he was about fifty-two years of age, and of about thirty years' Indian service. During late years he had suffered much from chronic diarrhœa and anæmia; a recent visit to Europe had, however, somewhat reinvigorated him. He was of spare, but active figure, and of regular and temperate habits; very intellectual, and much given to study. The duties of his office were of an important and responsible nature, and just before the illness for which he came under my care, they had been unusually onerous. He had, moreover, suffered much anxiety of mind and domestic affliction, from the illness and death of a very near relative. He had lived alone, and had almost entirely secluded himself from society since his return from Europe some months previously; but lately, his friends had induced him to go out a little, and he had apparently enjoyed the relaxation and change. It was on the 8th June (the weather being intensely hot), that I was called in, in passing his house; "he had just had a fit."

His servants, and one or two of his friends who saw him the day before, say he had been perfectly well up to 7.30 or 8 that morning. He had written a note early that morning just before he was taken ill, which was quite correctly worded—a note written to me, I imagine, just when he felt the attack coming on (and of which I append a copy); No. 1 was not so correct.

His servants say that he was lying on his couch, when they suddenly saw that he was convulsed on the right side, and that, on going up to him, he was quite unconscious. It was 10.15 A.M. when I saw him; he was lying on his couch with his eyes closed, but opened them directly I spoke, and appeared to recognise me; he then began to talk incoherently. He appeared not only to be unable to collect his ideas, but also to have lost the memory of words; he kept repeating one, which resembled "played." This came in as the second or third word of every sentence he tried to give utterance to, and he spoke very fast. For example, after replying to my question, "How are you?" he said, "I am better, I have played" (then incoherence),—"I don't know what brought this on,—I have paid, played, played;" and then again he became altogether incoherent. His pulse was 120, and the radial arteries felt rigid. The cardiac sounds were natural, perhaps rather weak. I had repeatedly examined his chest before the attack, and the urine had also been frequently analyzed, and nothing abnormal detected. Having lost a near relative from Bright's disease, he had been curious on this subject, and was constanly directing his attention to the state of his kidneys. The head was cool; the face pallid; no paralysis; no alteration in the tone of his voice; articulation was perfect; his tongue was clean; his bowels had acted freely that morning. His servants say, they thought him very well when he got up, and they were surprised to see the convulsions. They say, he was not hot at the time, and they noticed particularly that it was the *right* side that was convulsed. Ice had been applied to his head before I arrived. The urine was examined, and found to be free from albumen. It was acid in reaction, and of normal sp. gr.

It occurred to me that the great heat of the weather—thermometer over 90°—had something to say to the attack, which probably indicated degenerate cerebral vessels, perhaps an embolism in one of them; or that there had been some slight hæmorrhage, or congestion, or transient interference with the cerebral circulation, and probably great general exhaustion of the nerve-centres.

I ordered chloric ether and acetate of ammonia, and enjoined perfect rest and quiet; ice to be applied to the head if it became hot, beef-tea to be given occasionally, and the bowels to be acted on by a simple enema. I saw him again at 4.30 P.M., and he looked pretty well; he replied in a word or two to every question, but immediately lapsed into a state of incoherence; he did not recur to the same word that haunted him in the morning, but he substituted his words, and seemed totally unable to grasp the one he wanted. He evidently understood all that was said to him, and tried to answer. A friend asked him to go and stay at his house; he thanked him, and was able to say he preferred remaining where he was; but he was quite unable to continue the conversation, and became incoherent. I left instructions that he should be well watched, and that beef-tea and the medicine should be given regularly.

*9th June.*—He is in much the same state; pulse about 120; temperature of body somewhat high. He replies to a first question intelligently, but soon lapses into incoherence. I asked him to read; he took the book and pretended to do so, but it was the most incoherent jargon; all the time he looked quite intelligent. He has taken some nourishment, and is said to have slept. But for his shaven head, he looked fairly well. Cold had been applied to the head, and his bowels had been relieved. He was attended by a careful sick-nurse. In the evening I found him much the same; no improvement in his speech. Dr C—— had seen him with me in consultation at 1 P.M.

11*th*.—He remains much in the same condition; pulse from 110 to 120; skin cool, perhaps slightly feverish at times; the bowels act regularly. Takes readily all fluid food that is offered, and sleeps well. He is very quiet, tractable, and gentle; does or attempts all that he is asked to do. He walks with a peculiar gait, the body being bent forward; this is merely an exaggeration of his ordinary carriage. His tongue is slightly coated with whitish fur, and there is a peculiar and somewhat offensive odour in his breath. A small blister had been applied to the nape of the neck, which has risen well, but he does not complain of it in the least. He seems quite tranquil, and even happy; appears to recognise his friends, but he cannot tell, or rather, perhaps, he cannot remember, their names, or the words he wishes to say to them. Yesterday I asked him to name one of his friends who came into the room; he smiled and said, "Oh, that's go-up," and then he muttered some unintelligible words. He can reply to a first simple question, such as, "Have you slept or eaten well?" He answers, "Oh yes," or "No," as the case may be; but the next question, however simple, puzzles him completely, and the reply, for he tries to answer, is the most incoherent nonsense—words without connexion or meaning. I asked him to read yesterday, and gave him a book; he looked for his spectacles, put them on, then looked long and earnestly at the book, muttered a few words, and put it down. I then asked him to write a note; he sat down at his writing-table to do so, put on his spectacles, took pen and ink, adjusted the paper, and sat looking at it. Then, after about a quarter of an hour, repeatedly making efforts to begin, and saying, "I can't write, oh, that's just it!" he scrawled three figures of 8. To-day (the 11th) he read a few words correctly, and then became incoherent. He sat down to write at my request, and after about twenty minutes' delay, he produced the note No. 2, and then seemed so exhausted that he was glad to go and lie down. It is difficult to say how far he knows what he is doing. In the midst of the simplest reply to a question, he puts his hand to his forehead, appearing to try in vain to recall the word or idea he wants. For example, "Have you done so and so?" "Oh, yes!" "Do you like it?" "Oh, yes!" "Why?" "Because I—I—I can't work a bit, because it's a tight height." I have directed that he shall be very closely watched day and night, but he is tractable and gentle in the extreme. Nourishment with a little wine to be given frequently; an aperient when the bowels are confined; perfect quiet; the head to be kept cool; the feet warm, they are sometimes cold.

12*th*.—He seems rather better to-day: pulse 84; skin moist; head cool; bowels freely opened. I tried him with reading and writing; he read a few words correctly, but others he changed altogether. His writing is appended in Nos. 3, 4, 5. He answers questions pretty well, and looks as if he understands what he wants to say, though he is unable to remember the words he requires. He saw me looking at some numbers of "Good Words" lying on the table, and said there was something in them that was very good, but he could not remember what it was, or who wrote it; but he took one of the numbers up, and opening it at Gladstone's article on "Ecce Homo," then said, "Take it with you." All this was said as by one in perfect health, but he lapsed immediately into incoherence. He has eaten and slept well; is in good spirits, and answers cheerfully to any question. The same treatment continued.

*13th.*—He looks better; is sitting up; slept well; and is taking food freely. Had a pint bottle of claret, and a glass or two of sherry yesterday. I asked him if he had read the newspaper, and he replied, "Oh, yes; Eyre! Eyre, Chief Justice." He then took up the *Englishman*, and read that "the Chief Justice and all the *budges* (judges) had done so and so;" he made one or two mistakes, but on the whole had more command of words than he had yesterday. He remembered my name, and mentioned it several times, but he could not manage that of an intimate friend who had just then come in to see him. I asked him to write a note, and he at once cheerfully sat down to do so. The result is appended (No. 6). There is also a memo. of what he wished to have for tiffin (No. 7), and dinner, and an order that was to be sent to his wine-merchant (No. 8). His skin and head were cool; pulse 84; bowels open; much less of the peculiar odour first noticed in his breath. Altogether, he looks much better and stronger; is cheerful; and walks with a less stooping gait. To-day I ordered a quinine mixture with tinct. nucis vomicæ.

The blister on the neck is still open. He takes a generous diet, and one pint of claret daily. On the evening of the 13th he was evidently better. He had written an order to his wine-merchant, and some other notes. He read several lines with few mistakes; he seems much interested in doing this, but soon gets tired, and then he becomes quite unintelligible.

*14th.*—He is better this morning: pulse 80; has slept well; no heat of head or body; reads very well, miscalling only a few words. Talked quite naturally about many things, and especially about his illness; remembered being taken ill, but could not describe his sensation; remembered people also who came to see him, and the days on which they came, but could not always remember their names; even whilst talking he forgets words, or substitutes others of a similar sound; at the same time he appears conscious of his defect of memory. He wrote a note (copy appended, No. 9) to a friend; he remembered his name, and appeared much amused that he did so. It had been stormy during the night, and this was evidently in his mind when he wrote; notwithstanding that he spoke so well, the wording of his written memo. No. 10 shows how far he still was from health. I should note that the handwriting from the beginning has been almost as steady and firm as when in his usual health. Dr C—— saw him again with me to-day. He read and wrote for us; the reading had few mistakes, those mostly at the end of the sentences; the writing not nearly equal to the reading. He talked quite naturally on many subjects, and his general aspect was that of great improvement.

15*th.*—He seems to be doing well. He read a telegram in the paper, and commented fairly on it, but made several mistakes in his words; read part of a book equally well, and wrote memos. (Nos. 11, 12) about his food; his writing falls far short of his reading or conversation. His physical health is good; bowels regular; pulse 80; temperature of body normal; his memory, in some respects, is not so good as it was a day or two ago; he could not to-day remember the names of common objects, such as a bell, a book (the latter he called "bok"), a paper-knife, or his intimate friend's name, but was quite sensible of his defect of memory, and smiled as he alluded to it. He takes his food well, and half a bottle of claret daily.

16*th.*—He is much the same, with memory, if anything, rather clearer. He reads with few mistakes, but his writing (No. 13) was not equal to his reading; he has a fair appetite; slept pretty well last night. It should be noted that during the last ten days rain has fallen, and that the atmosphere has been much cooler, which has been in his favour.

17*th.*—He is improving; had a good night; tongue clean; pulse 80; blister healed. He read an advertisement in the newspaper quite correctly, and spoke well, with occasional mistakes, of which he was quite conscious. I have cautioned his friends and the nurse against allowing him to sign or write cheques or letters.

18*th.*—He continues to improve, and reads and writes (No. 14) better, forgetting fewer words. His physical health is otherwise good.

19*th.*—He continues to improve; conversation perfectly natural; reading almost without a mistake; writing (Nos. 15, 16) improved, but still not correct. An ordinary observer would now probably not notice any peculiarity in his conversation.

21*st.*—Doing well; speaks almost quite correctly. In reading he occasionally mispronounces a word, but seems aware that he has done so.

22*d.*—He is doing well; speaks and reads correctly, or nearly so; writing (Nos. 17, 18) improved, but still not perfect. Does not seem to be in quite such good spirits as he has been.

27*th.*—He is quite convalescent; has been out driving. His conversation and reading are now nearly perfect. He occasionally forgets or substitutes one word for another. He has a tolerably distinct recollection of all that has happened, and remembers being taken ill, but has no recollection of the fit. He is to leave India for Europe by an early steamer.

*3d July.*—He has left by the steamer, in all respects much better, but still he occasionally forgets a word.

He arrived at home safely, and for some time was much better for the change. Subsequently I heard, but without details, that he had some recurrence of the symptoms; the result of this attack I have not heard, but I believe he is at present (September 1869) alive and well.

The following are specimens of his notes and memoranda, written at my request during his illness. The writing generally was as good and firm as though he had been quite well. There are peculiarities which cannot be represented in print; often the initial letter of a word is obliterated and re-written, as though he had hesitated as to what letter he should use; and many marks and letters, that are of necessity unintelligible, are left out as though they could not be imitated.

A gradual improvement in each note may be observed, but the composition was always inferior to his manner of expressing himself; and when he wrote the last of these notes, his conversation was almost without a mistake. Most of these memoranda were signed in his usual bold and firm handwriting; his signature, indeed, alone would not have betrayed any defect. But the notes themselves show that, as Dr Bastian would say, the condition of agraphia continued after that of aphasia had well-nigh passed away.

No. 1. *8th June.*
Please come wall and see me. Y.
8th August.

No. 2. *11th June* 1868.
I am so bad to-day, abad, all abad. Yesterday all ab—abelegu—all bide—blessed, blesse tut.
Js. Js. Js.
P. P. P. P. P.

Bless, bless.
Bless: 11, bless 11, blesse.
P. B. B. B.
P. P.

No. 3. *12th June*, 8 A.M.
Wednesday.
I should nisbly a wark of blacking a boik of book if I ask.

No. 4. *12th June*, 4 P.M.
Have any one a dundred any brere wone.

By belief was very ill. A dwllng; so derely a darĕf; a waif all along light. At about a enayty brefest a baturfent about after a dayfeet about first day.

No. 5. *12th June also.*
I am better, he is better, but ill a ill my bill a better ill i bill my better is a bouller ill I——

No. 6. *13th June.*
It was bloying all the well, it was barring in bottle, it was sainty. It has been raining all the night; I have been very.

No. 7. *13th June.*
Bacon. Begs. Begels. Spiripe Scink. Soups. Spitch Cock. Spinciple.
*This was what he wanted for tiffin.*

No. 8. *13th June.*
Beshonath Law.
Claret wine as usual. Some for my tervan.

No. 9. *14th June.*
You we have had an awnully lot of both, during last night it was training during last night, the train was for-fully getting it.

No. 10. *14th June.*
Swit Bread. Dinner. Soup. Spick lock. Brodline.

No. 11. *14th June.*
I heems to have been pretty well and am as just as you are all. I am better than I was, but I hope in a few days I shall be able to do so as I well can.

No. 12. *15th June.*
Something about two o'clock a bed of mutton to read. Then at night, I will have something to read at nice I will have some wine for me.

No. 13. *16th June.*
It has been wet than it used to be. There is very little to be done for all the Englishmen to be said.

No. 14. *18th June.*
I have been seeding the paper, and have been reading about all the news.
Kidneys for breakfast.
Spitch cock. Soup. Roast mutton.
Wine. Bordeaux.

No. 15. *19th June.*
Morning. Eggs and bacon.
Tiffin. Mutton chop. Soup. Mutton or beef.
Bordeaux.

No. 16. *20th June.*
We have ordered fish and omelet.
There is nothing ordered for tiffin, as nothing is yet settleded.
Soup. Beef or Button.

No. 17. *21st June.*
I am very much better than I have been for the last few times. I have ordered some breakfast, but nothing yet for tiffin and dinner.

No. 18. *22d June.*
I am quite well, and I have nothing to bother me about my head at all. I have not heard what we are to have in tiffin or T. Tinner. The T breakfast we are to have fish and omelet.

I have recorded this case in detail, as it illustrates some points in the pathology of the disease that have been much discussed by recent observers. The symptoms were exactly those described by Trousseau and others; the loss of memory of words, even whilst the intelligence was comparatively good, was remarkably shown during his recovery. The incapacity to write correctly whilst he was able to express himself almost clearly, was very illustrative of that phase of cerebral disease which has been termed "agraphia" by Dr Bastian.

The convulsion on the right side, at the commencement of the disease, so far supports the views of Dr M. Dax and others who localize the cause of the disease in the left cerebral hemisphere. Happily, the opportunity did not occur of ascertaining whether in this case the views subsequently promulgated by Drs G. Dax and Broca, as to the precise localization of the lesion in the anterior and outer portion of the middle lobe of that hemisphere, near the island of Reil, and consequently close to the posterior portion of the frontal lobe, were correct. His recovery without paralysis, and the almost perfect restoration of the power of speech and writing, seems, I think, to point to disturbance rather than functional structural change; though the prematurely aged appearance, the rigid arteries, and other general indications of adipose and atheromatous degeneration, suggest the possibility of disturbed cerebral circulation, and point, perhaps, to the middle cerebral artery as the seat of some transient interruption of circulation, or the temporary presence of a thrombus or embolon, whose origin is traceable to degenerate vessels. It is possible that congestion, or slight hæmorrhage, may have occurred from similar causes; and as the central ganglia were probably only slightly compressed, the symptoms, after the convulsion had passed, were confined to what was due to changes of a transient nature that had occurred in the left anterior central lobe.

It is possible that this may have been one of the Protean forms assumed by cerebral exhaustion, the result of long residence in a tropical climate, exhaustive disease, and an overwrought brain; and that structural disease, in the conventional acceptation of the term, may have been absent; the pathological explanation being sought for rather in dynamical than material changes. In any case, it is impossible not to regard the future of one so affected with anxiety; and the chief practical lesson to be derived from such a case is the avoidance of a too protracted residence in a climate where we were not intended to live, as well as the early acceptance of those warnings which usually precede the breaking-down of the vigour and force of the nerve-centres generally, and the timely relaxation from oppressive mental work, as well as absence from the exhausting effects of a hot, damp, and malarious Indian climate.

Very little has been said about treatment; as far as the administration of drugs went, very little was needed. Rest, proper nourishment, and the absence of all excitement, were the conditions necessary to enable nature to repair the mischief, and restore the diminished power. Due attention was paid to the state of the bowels, and the head was kept cool, if ever the least increase in temperature suggested excitement of the circulation. A certain amount of wine was allowed with the food; for although a very temperate man, he had been accustomed to take a certain quantity of stimulants, and this certainly was not the occasion on which to withdraw the accustomed supply. A blister was applied to his neck, but I doubt if it was of any service, and I believe now he would have done just as well without it. I have strongly urged that he should never return to this country, and that his brain should not be overtaxed with work of any description.

## ARTICLE VII.—*On the Treatment of Diabetes by Milk, etc.: Being a Clinical Lecture delivered at the Edinburgh Royal Infirmary.* By GEORGE W. BALFOUR, M.D., F.R.C.P.E.

GENTLEMEN,—The first case to which I shall direct your attention is one of diabetes,[1] which has been discharged very considerably improved; no very wonderful event, for it often happens under various plans of treatment. Bromide of potass, full doses of opium, 14 grs. a day, and an alkaline treatment, the basis of which is citrate of ammonia, have all been found, in our own experience, to have considerable success, the latter being specially useful as a never-failing remedy for the thirst; and this success has been attained without much, sometimes without any, special attention to diet. Restriction in diet is, as you well know, the basis, the *sine quâ non*, in the treatment of all diabetics; but I have hitherto endeavoured to reduce this to its lowest limits, and, if possible, to ignore it, for this reason, that restriction in diet is for all an irksome burden, and one which hospital patients, with their limited intelligence, cannot berelied upon faithfully carrying out; while, even if they did so while in hospital, it would be vain to expect them to continue it after dismissal; for how could they afford to get gluten or almond bread, large quantities of animal food, washed down by soda-water and claret? Quite recently, however, an article of food—milk—has been proposed for the actual cure of diabetes, which has hitherto been looked upon with something more than suspicion, and has actually been tabooed to diabetics by a great authority, Dr Pavy, chiefly because it contains 3–6 per cent. of a saccharine principle, lactin; but partly, also, because of the unfavourable effect it seemed to have had upon a patient of his while under careful observation; the sugar rising from 1000 to 2000 grs. per day, and falling at once to 900 on the milk being given up. In this case, however, the milk was not given alone, but in conjunction with a full meat diet, and with a large amount of suet. Now, it is consistent with my own experience, and it has been also found by others, that a diet restricted to milk alone, and a diet composed partly of milk, are very different things in regard to the cure of disease; and many ailments which will yield to the one are wholly uninfluenced by the other. Milk, as a curative article of diet, has been very highly extolled from the earliest times; but, in recent days, it has found its most enthusiastic exponent in Dr Karell of St Petersburg, whose original paper in the *Edinburgh Medical Journal* for August 1866, p. 97, I commend to your attention. There you will find the whole history of this mode of cure, and several very remarkable cases by Dr Karell himself, selected from a total experience of 200 cases. I need not stop now to inquire how the milk acts, because beyond that it acts as a peculiar system of diet which means nothing, we know nothing; it is something more to the purpose that it is a diet procurable by the poorest, and one which can be readily taken and persevered in by all ranks and classes for months or years. The great Professor Black adopted it for many years before his death, because he thought it ought to be the natural food of an aged man commencing his second childhood: he died with a bowl of milk in his hand, and so quietly, that he did not spill a drop of it. I have known it successful in curing a very severe asthma, which had for years resisted every kind of treatment with the greatest obstinacy, and several very obstinate cases of general ill health; of late it has been brought before the profession by Dr Donkin of Durham, as a cure for diabetes mellitus, for Bright's disease, and for other forms of imperfect and deficient nutrition. In our own case, we had every reason to be satisfied with the results obtained. The patient was a woman named F., admitted 14th December, five months ill, and considerably emaciated. Her age was 51—so far favourable, because the older, within certain limits, the more amenable patients usually are to treatment. On her admission she passed ℥240 of urine, sp. gr. 1043, and drank ℥100; she was kept under observation for five days, being placed on full diet; her urine, during this period, varied from ℥240 to ℥174, sp. gr. always above 1040, the amount drunk varying from 5 to 6 pints. On the 20th of December she passed only ℥174, but on the 18th she had done the same; while on the 19th it again rose to ℥240. Obviously, therefore, there had been on the full diet alone no steady improvement, and no guarantee, nor from past experience any likelihood, that it would remain at that low figure. On the 21st, she was placed upon 6 pints of skim-milk for her sole diet, no other article of food to be taken; that day her urine fell to ℥120. She required no extra drink. On the 22d, she passed only ℥110, and the amount continued to fall till the 26th, when it was only ℥90, but still remained at the sp. gr. of 1040. She was dismissed on the 30th ult. at her own request, in consequence of domestic affliction. Up to her dismissal her urine continued steadily at the ℥90, but fell in sp. gr. so low as 1033 the day before she left. Obviously this woman was not cured, but she had certainly made a very remarkable improvement during the short time she was under treatment; and had she remained longer with us, I have no doubt that she would have been cured. As it is, her treatment, involving only a diet at once simple, easily procurable, and inexpensive, can be readily carried out at home, and I daresay she may there succeed in perfecting her cure. This treatment does not exclude the employment of opium, of iron, or quinine, or any other remedies otherwise considered appropriate to such cases; indeed, they may be very properly conjoined—in our patient they were not given, because not required. From two to three months is the period usually stated as sufficient to cause the complete disappearance of sugar from the urine; and then the diet may, if desired, be rendered more solid, by the detraction of some of the milk, and the addition of butcher's meat and green vegetables. It is doubtful, however, whether diabetic patients, even though apparently cured, can ever return to ordinary diet. Present experience of such cases is too limited to afford any data at all in regard to this. When the milk diet is employed in the cure of other forms of ill health, it is often conjoined after the first few days with a small amount of solid food; and what such patients relish most is plain bread with salt, or a salt herring, the mawkish sweet taste of milk inclining them this way. In diabetes, the thirst present usually makes it better to give the patient his full allowance of milk, and let him divide it over the day as he pleases. For other classes of disease the amount of milk must be at first restricted to two or three pints in the day, to be taken in divided portions at regular intervals. Except in the case of ulceration of the stomach, in which, from the great irritability and tendency to vomit, as well as the absolute necessity of keeping the stomach empty, and passing the fluid food through it to be digested in the intestines, it is absolutely necessary to give the milk in such small doses as ℥ss. every half-hour; or, in serious cases, where there has been much hæmorrhage, or where great irritability is present, even only once every hour; and such cases—kept, of course, in the recumbent position—will not only live, but actually thrive on this meagre diet.

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[1] The diagnosis, pathology, and ordinary treatment of this disease had already been fully treated of by the Senior Physician, and were therefore omitted, as involving needless repetition.

The next case to which I shall refer is one of a patient in Ward XIII., now in the Convalescent Hospital, affected by a peculiar spasmodic affection chiefly of the sterno-cleido-mastoid muscles, producing a continual nodding movement of the head, which never ceased except during sleep. Similar spasmodic affections are of not very infrequent occurrence, affecting frequently only one side, many such cases being related by Sir Charles Bell, and they do recover under a variety of treatment, which may be supposed to act in various ways. In this case the disease, after having for long resisted various powerful remedies, ceased so immediately as to leave us no choice but to believe that the remedy last selected had an actual and powerful effect upon the diseased nervous centre—an effect no doubt aided by the great improvement in her general health which had taken place during her residence in the ward. She was left in the ward under treatment when I took charge in the beginning of the session. At that time she was taking the succus conii in doses of six drachms, three a day. This remedy, you are aware, has been held by the researches of Dr Christison, and the more recent investigation of Dr Harley of London, to be a most valuable remedy in such cases, being said to induce exhaustion of the nervous energy of the spinal cord and of the muscles, commencing with the voluntary muscles. In this case it was given in continuously-increased doses, till so much as nine drachms were taken three times a day, with no effect beyond an occasional feeling of giddiness. It may be that the plants were ill-selected or the remedy ill-prepared; but, at all events, it was the best succus to be procured in Edinburgh, and quite as good as could be obtained anywhere else. The results of this experiment tend, therefore, like others, to show that no reliance can be placed on this drug—it seems to be nearly if not quite inert. Before trying any other drug, it was of course advisable to have an intermission to allow the conium to pass wholly out of the system. This intermission was very profitably occupied by a trial of the spinal ice-bags of Dr Chapman, the great neuro-therapeutist, as he styles himself. You are aware that Dr C. claims to be able to modify at will the circulation through every part of the body, and thus to remedy every morbid condition depending upon irregular or defective distribution of the blood, by the simple application of dry heat or cold down the spine, or along each side of it, so as to influence the spinal cord itself, or the deep-lying sympathetic ganglia ranged along the sides of the spinal column. These are grand problems to be so simply solved. In our case the matter was apparently simple enough, and readily enough to be attained, if the means were efficient. There was obviously an excited, hyperæmic condition of some small part of the motor columns of the upper part of the cord; and if ice applied over the spinal column affects the interspinous circulation, as Dr Chapman says, there seemed to be no difficulty. Apart from the practical results of such an experiment in therapeutics as we now instituted,—which, if successful, is liable to the doubts which involve all experiments in therapeutics, and, if unsuccessful, is always open to the objection that it has not been properly or efficiently carried out,—it has always seemed to me somewhat singular to expect the direct action of a cold, insufficient to freeze the skin, to extend deep and modify the circulation of a part placed within a well-lined bony cavity carefully prepared for the purpose of protecting its contents from such influence; and this, though we know that when the skin is actually frozen, the circulation and state of the nerves and other parts beneath it remain altogether unchanged, as we learn from cases of operation performed under such circumstances. The part may be modified by being frozen all through, but not otherwise; that, of course, we dare not do, even if we could, to the spinal cord. Cold occasionally modifies the circulation both in the spinal cord and brain, producing forms of delirium sometimes indistinguishable from drunkenness, and many forms of paralysis, both local and general; but in these cases it acts on the whole surface of the body, and has been shown, by the experiments of Brown-Séquard and Tholozan, to act by contracting the small arteries reflexly. That, however, is a very different matter from Chapman's idea of influencing the bloodvessels of the cord and ganglia by the direct application of cold and heat. These paralyses are best treated, not by hot applications to the spine, as they would be were Chapman's theory true, but by warmth to the surface of the body—warmth applied to the parts primarily affected. You will not, after what has been said, be surprised to hear that the direct application of pounded ice to the upper part of the spine, continued persistently for many days, remained without effect. I confess I was not disappointed. Some years ago, in a severe case of epilepsy, a careful and persistent trial of the application of ice to the spine was made for many months without any effect whatever, though Dr Chapman claims this disease as one most readily cured by these means. Both theoretically and practically, therefore, spinal ice-bags seem to be unworthy of any confidence. I do not say cures never follow their use; but I may be permitted to doubt the cure being a true result. This little experiment having failed, we again betook ourselves to the more ordinary materia medica, selecting this time that substance which from its physiological action seems to promise most help in all cases of anormal muscular movements—Calabar bean, which is known to act physiologically on the spinal cord, by destroying its power of conducting impression, and also to produce primarily paralysis of the voluntary muscles: some such action was precisely what we wanted in this case. She was accordingly prescribed 5 gr. of the Calabar bean in powder, three times a day, with the effect of giving immediate relief. In a couple of days she was quite well; the movement had entirely ceased. The remedy was continued for some time, and she was sent to the Convalescent Hospital cured.

You may remember, at the close of my last lecture, that I exhibited to you the bones of a horse affected with rheumatoid arthritis. The case to which I then referred is still under treatment, though improved; and I purpose referring now more at large to the subject. I brought these horse-bones the other day for several distinct reasons—1st, To show you well-marked examples of eburnation on a larger scale than we ever have them in human joints; 2d, To show you that this eburnation in the horse, at least, is not confined to the surface of the bone—is not, therefore, produced, as has been supposed, by mere consolidation and polishing produced by friction, as you would polish a piece of hard wood or dead bone; for you remember, in the one bone, the os naviculare —a sesamoid bone, not exposed to friction or pressure at all—this eburnation had extended throughout the whole substance of the bone, showing that probably the process is an integral part of the disease, and does not depend on friction at all,—as, indeed, in living bones we could scarcely expect would be the case; and, 3d, To show that horses, who live so very differently from man, both as regards food and drink, and who never have gout, are yet commonly affected by a disease which produces precisely the same pathological results as rheumatoid arthritis in man—thus affording additional proof that rheumatoid arthritis has nothing gouty in its nature; and showing that, to obtain anything like a proper and profitable pathological history of this disease, we must include the lower animals in our survey. As yet very little is known of rheumatoid arthritis, and yet it is of importance to know that little; because it is of the utmost consequence to recognise both the acute and chronic disease, the treatment being so dissimilar from that of the diseases with which they are apt to be confounded—chiefly rheumatism and gout—but which may also in the early stages include locomotor ataxy and Bright's disease, both of which are frequently associated with pains which might be mistaken for those of rheumatoid arthritis—an old disease, long known by those skilled, but little recognised generally by the profession. Mr Adams of Dublin has published the most important monograph upon the subject, bearing specially upon its morbid anatomy—he called it rheumatic gout, recognising it as distinct from both rheumatism and gout, and yet supposing it might be a compound hybrid. Haygarth long ago termed it nodosity of the joints—frequently affecting the hip-joints, it has been termed morbus coxæ senilis; and Garrod has called it rheumatoid arthritis, recognising it as an inflammatory affection of the joints, the nature of which somewhat resembles rheumatism in its early stages, though it is not truly rheumatic; therefore he calls it rheumatoid. Patient is a man in Ward V., who came in with an acute attack, supervening on the chronic affection, of some standing; it is much more common, however, in women than in men. Haygarth states, that having observed the disease for twenty-six years, he had only once seen it in a man. It may attack all ages, from the youngest to the oldest;

it is common in the families of the consumptive, and all who are from any cause debilitated are more liable to it; it is comparatively rarely seen in our wards, and yet the treatment and prognosis are so different from allied affections, that it is of importance that you should recognise them. In acute rheumatism death rarely occurs, and only as the result of implication of some internal organ—perfect recovery being the rule; crippling of the limbs from an attack of acute rheumatism being unknown. Gout, too, is rarely fatal, except in old age, or from internal and, except in advanced age, somewhat rare complications, while the joints, though often deformed by urate of soda, are rarely crippled; but rheumatoid arthritis, though perhaps never fatal, yet always cripples the patient more or less, and deforms him very much. It, I believe, is the chronic rheumatism—so called—which deforms; and it deforms more often than it ought for want of proper treatment. The diagnosis in the case in Ward V. was readily made: his former attacks rendered this comparatively easy; there was the characteristic swelling of the ends of the metacarpal bones,—a swelling which, however great in appearance during life, is not always attended by much actual enlargement of the bone, as you may see (plates of bones and of hand exhibited); in one hand there was also a well-marked tendency of the fingers to incline towards the outer or ulnar side of the hand—a tendency quite characteristic of this affection, which is also well seen in a less advanced case now in Ward XIII. for another complaint. The man is now a good deal better, and this tendency is now considerably lessened; but, along with these proofs peculiar to the disease, we had also the absence of symptoms peculiar to the other diseases, with which it might have been confounded. Thus, there was comparatively little constitutional disturbance, no continuous, sour-smelling sweats, absence of any tendency to heart affection, absence of any shifting from joint to joint, considerable swelling of the joints themselves simply, and not of their exteriors,—all distinguishing it from acute rheumatism. The diagnosis from gout presented in this case no difficulty whatever; the attack presented not the slightest similarity to this disease. Chronic rheumatoid arthritis is, however, sometimes with difficulty to be distinguished from chronic gout, the chief diagnostic results being the absence of tophaceous—urate of soda—deposits in the lobe of the ear, tips of the fingers, or in the bursæ over the olecranon. There is also no material alteration in the urine; and in obscure cases a blister may be applied, when the serum will be found free from uric acid, which in gout it never is. It is of importance to remember these points, for I have seen cases subjected to various treatments under the supposition that they were merely chronic rheumatism, and others who were dieted and drenched on the theory they were gouty. Rheumatoid arthritis appears to be a disease arising from exposure to cold and damp, but only in a person constitutionally predisposed by debility from some cause or other. But, truly, we know nothing certain in regard to this, and we require to have our knowledge of human pathology collated with that of the horse before we can form any rational theory on the subject. The blood, the urine, and the perspiration have all been analyzed with merely negative results, and yet the morbid anatomy of the disease is perhaps the richest and the most extraordinary belonging to any complaint, the disease affecting, in its peculiar way, every bone and every joint in the body. In the early stage of the disease, there is found simply effusion of synovia, with vascular injections, signs of arthritis; at a later stage the fluid becomes absorbed, the capsular membrane thickened, and the cartilage ulcerated. At a very early period this cartilage splits up, and becomes removed apparently by a slow process of absorption, the whole surface of the joint being then denuded, the osseous surface becoming polished and eburnated, as in those specimens. In the ball-and-socket joints, as the hip and shoulder, the heads of the bone sometimes become polished like ivory billiard-balls, in the ginglymoid joints the eburnation puts on more the appearance of grooved striæ or patches; there is a growth of bony vegetation around the joint affected; the heads of the bones, especially in ball-and-socket joints, frequently becoming depressed, and so crushed down and deformed as almost to appear as if they were fractured. The interior of the joints frequently contains great quantities of foreign bodies of various sizes—sometimes cartilaginous, at others bony—soft with a hard exterior, or the reverse; these are occasionally but rarely found free, being most commonly attached to one another, or to the joint by a ligamentous pedicle. The prognosis of this disease is not very hopeful. Sometimes it is arrested, but most commonly it progresses without any intermission, and but very slight remissions, throughou all the patient's future existence—the nodes and bony vegetations gradually enlarging and spreading from joint to joint, without ever leaving that first attacked—sadly embittering by this gradual progress all the comforts of life, but not necessarily curtailing its length. The first patient Dr Haygarth saw labouring under this complaint, lived to the age of 93. In regard to the treatment of this curious disease, very little is to be said. If we consult Haygarth, he apologizes for knowing nothing about it: if we ask Sir B. Brodie, he is too politic to confess his ignorance, but tells us to place the patient on a careful system of regulated diet—partaking moderately of animal food, avoiding fruits, acids, raw vegetables, and sugar, and taking little or no spirituous or fermented liquors; also to make him take daily exercise, sufficient to further gentle perspiration—all very sound rules for health, but questionable as curative. In the horse we have an animal given neither to excess in animal food nor spirituous liquor, and daily perspiring at his work, yet no animal is more subject to this disease than he. Clearly diet has not much to do with it; indeed, the only patient whom I know to have made a well-marked improvement in regard to this complaint, is one who has never paid any attention to diet, but neither was she ever given to excess. She has gradually improved her position in life, is now able to avoid exposure, to feed better than she used to do, to take less exertion, and she has also ceased to be after the manner of women; all circumstances favourable to increased constitutional strength. Cessation of the menses is not, however, always a favourable circumstance in such cases, for in very many instances the disease first makes its appearance at this period; of course, in regard to this, much must depend as to whether the constitution have been previously improving or deteriorating. A warm climate, warm baths and douches, especially salt ones, which stimulate the cutaneous circulation, are all useful to such patients. My own experience has been that, as a rule, treatment is of little avail, except when by our treatment we can improve the health of the patient. Tonic treatment is therefore most available, and, of all the tonics, there seems to be none equal to full doses of liq. arsenicalis, 10-15 minims for a dose. In many cases, this, coupled with the external appliance of liquor iodi. to the joint affected, will be productive of great benefit. Alkalies, colchicum, and all depressants—all the remedies useful in rheumatism and gout—do harm. Cod-liver oil is sometimes useful, if there be a tendency to waste. Chalybeates are useful when there is anæmia; and these may be both combined with the arsenic. All mineral waters which act powerfully on the skin, kidneys, or bowels, are useless, and often injurious, but tonic chalybeate mineral waters may be useful; while M. Trousseau recommends iodine internally as a most useful remedy; more recently, the interrupted current, Faradization, has been recommended as a never-failing remedy. My own experience is that arsenic, with iodine application externally, are the most useful remedies in the acute form of the disease, while in the chronic form, hot douches and friction, with chloroform liniment, with suitable tonic regimen, give most relief. By far the most important part of treatment being, however, to recognise the disease early, so as to avoid increasing the malady by the use of remedies, which, however useful in rheumatism and gout, are altogether unsuitable in rheumatoid arthritis.

In Ward V. we have a man aged fifty, labouring under psoriasis of twenty years' duration. He has been repeatedly under treatment, and has been repeatedly cured, or apparently so; the disease always returning more or less rapidly. Psoriasis, as you know, is a disease which lasts many years, during which it undergoes many changes, and may even subside temporarily; it is therefore not always cured when it subsides under treatment; hence, in all cases, it is of importance to keep the patient under treatment, or, at all events, under observation for some time after the disappearance of the eruption, ready to treat the very first indications of its recurrence. A single attack of this disease, without any subsequent return, is a most exceptional occurrence; but there is this comfort, that even when it has existed for long, the general health remains unaffected, the greatest discomfort complained of being a more or less troublesome itching. Psoriasis consists essentially in a chronic, inflamed, hyperæmic, condition of the papillæ of the corium, over which the epidermic cells become accumulated. In the earliest stage—for all the so-called forms of psoriasis are merely different stages of one disease—these hyperæmic papillæ are isolated, and present the appearance of small papules of the size of pin-heads, elevated, and white from the accumulation of the dry epidermic scales—Psoriasis punctata. After a few days or longer, these papules get larger, as large as peas, and the disease is then termed Psoriasis guttata. The spots of psoriasis gradually enlarge at their circumference, becoming discoid in appearance—hence Psoriasis circumscripta or discoidea. Gradually these enlarging discoid patches meet one another and coalesce, their form thus becoming irregular, and the size various. The form of the patches being originally circular, whenever they coalesce, they are always made up of segments of circles, and present a somewhat serpentine appearance; hence the Psoriasis gyrata, a form and name which only indicates a protracted disease. While, when we have combined with this form, indicating a protraction of the disease, punctiform and guttiform patches, indicating a continuous outbreak of the disease, such a condition is the Psoriasis diffusa or inveterata. Even in the worst cases, however, with very rare exceptions, the skin is never universally diseased, healthy patches of greater or less size always intervening between the diseased spots. As the disease progresses from what has been termed the discoid variety, another change takes place—the scales separate and fall off from the whole eruption, or from certain parts of it, leaving bright-red, slightly elevated patches. When the scales only fall from the centre of the discoid patches, remaining adherent at the edges, this is what Willan has termed Lepra—no true disease, but a mere variety, in appearance only, of psoriasis. The centres of these patches occasionally heal, and become normal, surrounded by circles of scales, or, if these have fallen off, by bright-red rings. As these patches continue to spread externally and to heal internally, they give rise by coalescence with each other to many striking appearances. Every part of the skin, including the hairy scalp, is liable to this eruption. When the scalp is affected, the hairs may fall out, but they are not diseased, and look healthy. The lips and mucous membranes are never affected. Limited and isolated patches are occasionally found on the elbows and knees. The history and appearance of the disease is sufficient for its diagnosis, one great point being the total absence of moisture, the dryness and quantity of the scales. The appearance of the disease gives its own prognosis. The early forms—punctiform, etc.—forebode a tedious, because commencing disease; the later gyrated forms indicate a disease already yielding; when complicated by the early forms, however, as in the present case, we may prognose a protracted and obstinate disease. In very diffuse and obstinate cases, the disease may even be dangerous to life, which, of course, it usually is not. We know nothing whatever of the causes of this disease; its treatment, however, is simple, always gives relief, and often cures. There is one drug which hardly ever fails to promote at least a temporary involution of the disease, and that is arsenic; but this, of course, must be conjoined with other remedies needful to improve the health, which is usually out of sorts in such cases,—such as steel, cod-liver oil, or lime-juice, the latter remedy hardly ever failing to relieve the intense itching occasionally present, and always most distressing to the patient. The arsenic must be given in full doses; what I prefer is the liq. arsenicalis; Fowler's solution of the arsenite of potass in doses of 10 to 15 minims, twice or thrice a day, continued till the disease is cured, or intermitted for a short time, should itching of the eyelids or other unpleasant symptoms manifest themselves, which is only rarely, as there is usually a great tolerance of arsenic in such cases. In from two to three months improvement may be expected to commence; in cases of any standing, it seldom does so sooner—at least such has been my experience. Along with this internal remedy, which is our great stand by in such cases, and the form and mode of administration of which may be varied, I have been in the habit of employing zinc ointment, combined with chloroform, when much itching was present, or simply with carbolic acid, in the hope that it might prove a cleanly and agreeable substitute for the very dirty pitch ointment, which has long been known as a most efficacious remedy in such cases. Carbolic acid has, however, failed to do any service in such cases; and latterly I have been in the habit of employing an ointment composed of equal parts of creasote and wax, white or yellow, softened, according to the temperature of the season, by being mixed with lard or olive-oil. This is an ointment cleanly in its application, not disagreeable in its smell, and which seems to possess all the efficacy of the old tar ointment; and I can confidently recommend it to you as the best local application for such cases with which I am acquainted.

# Part Second.

## REVIEWS.

*Epidemic Cholera in the Bengal Presidency.* A Report on the Cholera of 1866-68, and its Relations to the Cholera of previous Epidemics. By JAMES L. BRYDEN, M.D., Surgeon, Bengal Army, Statistical Officer attached to the Sanitary Commissioner with the Government of India. Calcutta: 1869.

IN a review which appeared in the Number of this Journal for February 1868, we called attention to Dr Bryden's views upon the spread of cholera in India. The review was principally written from a short paper printed for the information of Government, and giving the leading generalizations which the study of the history of cholera in India had enabled him to reach. In the Report now before us we have those views matured and supported by ample statistical evidence. The Report is illustrated by coloured maps, which are especially useful to those not familiar with the geography of India. The usefulness of papers like this, printed for the information of Government, is no doubt increased by a statement of some of the evidence on which their deductions are founded; besides, they are not often studied, save by those willing to devote time and trouble to their perusal. Nevertheless, we suspect that it would be better for their writers to study greater compression, and to eschew repetition. We do not mean to say that Dr Bryden is more amenable to this charge than the authors of similar Reports which have come under our notice; perhaps, indeed, he is less so; but this is a common fault, which might be amended, both for the advantage of producers and readers of such documents. Of the value of the Report there is no question. It is well written, clear in expression, precise in description, and bears on every page marks of the greatest earnestness of purpose and keenest interest in the subject under inquiry. Dr Bryden has studied the history of cholera in India in a thorough manner, and applied his mind with intense effort to the solution of the intricate problems which the history of the disease holds out. As a record of the way of diffusion of cholera in India, the work has no rival. No one has, as it were, at one jet, sent forth so clear and powerful a light upon this obscure and difficult subject.

"As regards the data," says Dr Bryden, "from which the deductions are made. The field of observation extends from the Assam frontier in the north-east, to the valley of the Nerbudda and Western Malwa in the south-west; and from Pooree and Cuttack in the south-east, to the north-western frontier. The population of which the statistics are here employed, exceeds 160,000 daily under European medical supervision, and is representative of three distinct types. The European army furnishes the results for between 40,000 and 50,000 men, women, and children located within this area; the native army numbers 60,000 men; and the jail population of the Presidency varies from 50,000 to 55,000."

These statistics are reinforced by general observations on the health of the native population. The Report, exclusive of appendix, fills 244 pages folio, and it would be impossible with our space to do anything more than reproduce some of the views it teaches.

Dr Bryden holds that cholera is a miasm generated in the ground with the assistance of heat and moisture, and that it uses the air as its vehicle of diffusion. In all these respects it bears a family likeness to the malaria of remittent or intermittent fever; but is capable of much greater diffusion from the spot where it has been first produced. Though described by Charaka, the most ancient Hindu writer on medicine, it is only in comparatively recent times that cholera has assumed the terrible importance which it now possesses. The European soldier in India is peculiarly exposed to its destructive attacks, and one-third of the mortality of our white army in the Bengal Presidency is owing to this one disease. The endemic area of cholera is bounded on the north by the Himalayas; on the south by the Bay of Bengal; on the east by the hills beyond the Brahmapootra; and on the west by the hills of Rajamahal and Cuttack. This tract is traversed by two of the greatest rivers in Asia, and is exposed to the full influence of the monsoon; it has a very heavy rainfall, and bears the vegetation of a hot and moist climate. The inhabitants live largely on rice; but the prevalent diseases are not to be traced to the influence of diet, for there is abundant proof that no kind of diet will exempt different races or castes from the diseases of the climate. Though endemic in Lower Bengal, cholera has intervals of comparative dormancy, and is more frequent in the dry than in the rainy months, when its ordinary breeding-grounds are under water. That part of the Bengal Presidency which cholera invades as an epidemic comprises Behar, Nagpore, Central India, the North-Western Provinces, and the Punjab. In its periodical visits to these districts, it observes an astonishing regularity, frequently re-appearing in successive years at dates closely corresponding one to another, like the budding of the leaf, or the flowering of a plant, or the hatching of the eggs of an insect.

The assertion that cholera travels along the highways of communication is not borne out in India. To take, for example, the Grand Trunk Road, it is shown "that cholera is never conveyed along it from the eastern half of the Doab to the western, or in the contrary direction; that when the western half of the epidemic area is invaded, Lahore is reached from the south-east as soon as Umballa, or even, as in 1856, a month before Umballa is affected;

and that cholera had no power in 1856 or 1861 to pass beyond the Jhelum on its way towards Peshawur and Cabul. Some have endeavoured to advance the hypothesis, that because it will generally be found that the first cases of cholera which occur came from an affected district, the fact is sufficient proof of the introduction of foci of contagion. It has no such significance; it must follow as a matter of course, that the first cases occurring in any province shall be imported cases. When, as in 1856 or 1861, epidemic cholera continued chained down in the Agra district and in the districts south and west of the Jumna from May to July, it cannot be imagined that six weeks elapsed without some passenger on the great highways carrying cholera with him in the latent stage into the Meerut district or the Punjab. I do not know that such cases did occur; but it suffices to me to know, that within three weeks from the date of the epidemic, commencing to move with the setting in of the monsoon, the entire province was covered, and its population destroyed by tens of thousands; and I infer, that the same distribution would have occurred had there been no human being existing within the invaded area."

The re-appearance of invading cholera at particular spots at, or nearly at, the same days of the month, would seem to be connected with the striking regularity of the approach of the monsoons or periodical rains, which seldom fail to return every year about the same week. Dr Bryden has collected a number of observations tending to prove that a moist state of the atmosphere is favourable to the existence of cholera, and he believes that the wind is the agent by which the miasm was diffused.

If the air is very moist the cholera will remain hanging over a locality, but will not advance till the appearance of the wind, which moves it forwards. Dry heat arrests; a dry wind will drive it backwards. The oft-repeated assertion that cholera spreads against the winds is treated as altogether erroneous, or arising from a mistaken apprehension of facts.

"Some have sought to prove, that the prevalence of cholera bears no specific relation to the moisture of the atmosphere, because it prevails, and is epidemically spread, with very different degrees of saturation. As I have shown from the phenomena of diffusion over the eastern area in the spring, it is the absence of the dry heat of the west winds that makes an epidemic season, and a few inches of rain in May will determine an almost universal distribution of cholera over Rohilcund, Meerut, and the Punjab, as in 1862, 1865, and 1867."

According to Dr Bryden, it is the spring and summer rains which carry cholera in successive epidemic waves over the north-west of India, and it is the intervening warm winds which divide the spring from the summer visitation. Cholera requires the assistance of moist or rainy weather to travel into those parts of the Punjab across the Indus, and beyond the ordinary reach of the monsoon, In those years when the rains are deficient, the cholera is generally wanting in the Punjab and North-Western Provinces. But an increased rainfall does not necessarily lead to a more severe visitation of cholera. Dry deserts, like those around Googaira or Bikaneer, are habitually exempted from cholera.

The general impression in Europe is, that the prevalence of cholera cannot be associated with any particular kind of weather; and it is probable that Dr Bryden's views upon this point will be met with some distrust. Few people like to be led further than they can see; and India, to most men in Europe, is a country little known and little understood. It is, however, clear, from several citations in the Report, that the influence of moist weather upon the prevalence of cholera has been noted by some competent observers.

Having invaded a province, cholera, after dying away during the cold season, frequently re-appears over the occupied districts next spring, but this continued occupation must not be confounded with a renewed epidemic; it is due to the revitalization of the germs of cholera, which have lain dormant for a season.

"The revitalization *in situ*," says our author, "during the vitality of the same reproduction of a cholera distributed over an invaded area, is very much the same thing in homology as the revitalization which occurs provincially after the normal period of dormancy; after lying dormant for six months, vitality returns to the cholera seed sown over a province; in invasion it is usually a vital cholera that is precipitated, and this seems to be re-adapted for fresh manifestation within a fortnight after it is sown. It can be readily understood that many spots and special localities are far more favourable than others for preventing the decay of the distributed miasm, and for promoting its propagation, and are, in short, adapted to bring about localization, and consequent intensity of manifestation, even during the course of the current reproduction."

Nevertheless, a time comes when this epidemic loses its force, and dies away. In general, four years is sufficient for it to become entirely extinct, and it requires a new invasion from the endemic area to enable it again to commence its ravages. The history of different epidemics is shown occasionally to present very close parallels, and sometimes to be accompanied by similar meteorological phenomena.

Dr Bryden has carefully studied the character of those epidemics which reach Europe. Sometimes the disease travels through the centre of India to Bombay and the neighbouring coast, whence it passes to Arabia and Eastern Africa, reaching Europe through Egypt. At other times it enters in the course of the northern epidemic from the Punjab through Cabul. Dr Bryden does not claim that his views have yet gained a fixed and complete form. They are more or less rudimentary, and have been, during the years he has been attached to the statistical department, improved by continual correction and revision.

Medical science can unfortunately suggest little to check this mysterious messenger of death. It seems probable that an increased height to the barracks of our troops might diminish the number of seizures, and there is some evidence given to show that liability to cholera is much lessened in places surrounded by trees, in the same way as a belt of trees is known to guard a station from the entrance of the malaria of paludal fever. Dr Bryden, though admitting the occasional communication of cholera by human intercourse, assigns to this communication a subordinate part. While admitting that the Hurdwar pilgrims communicated the malady to some of those with whom they came in contact, he treats the cholera of 1867 as an epidemic whose appearance he foretold, and whose spread is attributable to the ordinary laws which carry the disease over the north-west of Hindustan. Those interested in such questions will no doubt consult his work for explanations on this point, and very likely they will not be entirely satisfied. The theory that cholera is spread by human intercourse alone, almost unrepresented in India, has in Europe made many converts. The mere desire of unity will influence men to try if all the effects cannot be traced to a single cause, especially when its occasional efficacy is granted. But there is a wide difference between admitting that a disease sometimes passes by infection from one body to another, and that reproduction in the human body is its only means of propagation. Experienced physicians have observed instances where influenza seems to have been introduced into a family by a new arrival, but no one ever dreamed that its rapid spread could be accounted for by any species of contagion. Since tubercle has been communicated to rabbits by inoculation, we may admit, without laying too much stress upon isolated facts, that the suspicions of former physicians that phthisis was occasionally communicated from one person to another may have been correct; but we know that this is a rare occurrence. Any one who would try to pass off such transmissions as a common cause of consumption would do much mischief. People talk of erring on the safe side; but the truth is, error is never safe. Those who exaggerate the danger of attending the sick are doing their best to add to the misery and loneliness of a sick-bed, which some day they may feel themselves.

If Dr Bryden gives the contagionists no assistance, he at least points out how they may help themselves. By giving the precise time when cholera may be expected to appear at so many different places, he shows them when and where to be ready to trace the communication of the disease from one person to another. They must perforce explain many of the statistical facts in a different manner; they must make the most of peculiar states of atmosphere and of variations of human susceptibility. They will, no doubt, contend that if dry heat destroys cholera, it also renders idle the contagious power of smallpox and typhus fever; and they will regard those arid tracts which Dr Bryden believes exempted from the absence of moisture in the air as being exempted mainly because the sparse character of the population renders transmission difficult.

There is little in this report bearing on the hypothesis of Pettenkofer, which seems framed more to get rid of objections than to answer known facts; but here is the experience of a medical officer who is worth listening to, Mr Scriven, who was surgeon in the General Hospital at Calcutta during six and a half years:—"It may be truly said that cholera is never absent from Calcutta, and at the changes of the seasons we used to have numerous cases of the severest kind. They were always treated on the south division of the large lower ward in the body of the building, which was not in any way shut off from the rest. I have often seen this ward full of cholera cases, with the attendants constantly employed among them, yet I never heard of any of them catching the disease, nor do I remember any of the other patients in the ward being infected. This southern part of the ward was so frequently filled with cholera cases that we never thought of taking any special steps to cleanse it, otherwise than by ordinary washing; nor do I remember any facts occurring at the hospital that would have led me to think such a step was necessary."

Now, suppose the General Hospital had been guarded, fumigated, and disinfected for six or seven years, with the highest industry of scientific fussiness, would not this very result have been hailed and quoted as a proof of the competency of science to check the contagion?

If we may venture to give favour to any theory, we would ask whether the theory of the parasitic origin of cholera does not more satisfactorily account for all known facts concerning its propagation, for its regular appearance at particular seasons, for its dormancy at others, for its occasionally occupying new geographical areas, and returning to particular buildings, or particular parts of buildings, and also for its at one time appearing to pass by infection from one individual to another, and at other times remaining incapable of so doing, as well as for its being transmissible to animals by inoculation? But as the microscopists have not been very successful in their attempts to detect the particular fungus, the suggestion is made in a diffident spirit.

There are many interesting topics handled in this Report, such as the effects of regiments going into tents to avoid cholera, and the necessity of knowing the character and type of reigning diseases, ere we pronounce upon the efficacy of medical treatment.

We are promised a report from Dr Murray, Inspector-General of the Medical Department, giving an appreciation of the different methods of treating cholera, drawn from his own observations as well as from those of other officers whose experience and judgment entitle them to attention. We neither doubt Dr Murray's experience nor his ability, but few have any faith left in the old stock methods of treatment. We should advocate a trial of the system of treatment propounded by Dr Chapman in his book, "Diarrhœa and Cholera." We are convinced by actual experiment that the circulation of the internal viscera can be modified to a considerable extent by the application of heat and cold to the spine; and a derangement of the circulation is undoubtedly one of the most formidable of the morbid phenomena characterizing cholera, and one which it is useful to combat. We have satisfied ourselves that the hot-water bag is useful in relieving dry congested conditions of the mucous membrane of the lung, and have witnessed the efficacy of ice in sea-sickness. The experience derived from treating cholera in this way is not large, but seems so far satisfactory as to demand further trial, and this could be more easily made in a city like Calcutta than anywhere else.

*Lectures on Surgery.* By JAMES SPENCE, F.R.S.E.; Surgeon to the Queen in Scotland; President of the Royal College of Surgeons, Edinburgh; Professor of Surgery in the University of Edinburgh, etc. Part II. Edinburgh: Adam & Charles Black.

IT was generally expected by those who knew the career of Professor Spence, that his "Lectures on Surgery" would become of more practical value as their publication progressed, and in Part II. these expectations have been realized.

Part I., devoted chiefly to the special forms of diseased action, seems to have given rise to considerable criticism, according as the author's views on the pathology of disease did, or did not, conform to the current opinions of the present day.

In Part II., which contains the diseases and injuries of special structures, Professor Spence is thoroughly at home, and he brings to bear on the important subjects of which he treats, the carefully treasured experience of many years as an hospital surgeon, coupled with more than ordinary opportunities for observation in private practice.

Where so many different surgical diseases and lesions have been ably described, it is difficult within the limits of a short notice to select what we may consider as peculiarly worthy of commendation, but we may, perhaps, direct the special attention of our readers to the lectures on the injuries and diseases of arteries, as furnishing an admirable resumé of the conclusions arrived at, both experimentally and through the examination of cases, by a number of illustrious and painstaking observers.

Professor Spence's own experiments, an account of which had not been previously published, have furnished most important evidence, in opposition to usually received theories, regarding the organic changes which take place in and around the artery after ligature.

They have shown that the steps, which nature takes to assist the surgeon in his endeavours to occlude by ligature an artery, are two in number:—1st, Deposit and organization of coagulable lymph within the vessel at the point of ligature, either with or without the rupture and curling up of the inner coats; and, 2d, Deposit of the same nature in the parts immediately external to the vessel. He has pointed out that the clot, so long considered the *sine qua non* for satisfactory obstruction, is in reality non-essential. These observations are illustrated very beautifully by coloured drawings from life.

The diagnostic differences between Caries and Necrosis, the pathology and treatment of these affections; the diseases of joints; the causes of displacement in various fractures, with the appliances for their rectification,—are all discussed with a freedom and ability which show how thoroughly he has made himself a master of the science and art of surgery.

Where his views differ from those in ordinary acceptance, there is no display of dogmatism, but his opinions are supported by fairly-stated arguments and confirmatory cases.

Throughout the whole of Part II. many most valuable practical hints may be picked up, not by the student only, but by the advanced practitioner, which will render comparatively easy what hitherto may have proved a source of much trouble or even disappointment.

While we have said so much in praise of these "Lectures on Surgery," we cannot but regret that Professor Spence has published the work in its present form. Coming, as it does, from the pen of the Professor of Surgery in the University of Edinburgh, we think the various subjects might have been more satisfactorily dealt with in a systematic treatise.

The form of lecture might have been reserved for clinical observations on the numerous illustrations of surgical disease, which have come under the cognizance of Mr Spence, and which, we trust, will some day be given to the profession.

It may be that no brilliant discoveries of novel modes of treatment are to be found in this book, but the reader cannot peruse any portion of it without feeling satisfied that every subject discussed by Professor Spence has been most searchingly tested, and that his conclusions may be unhesitatingly relied on, as the result of long-continued, careful study. It can be seen at a glance that the work is not a mere compilation, but the detailed experience of a successful practical surgeon, who feels himself justified in recording, for the benefit alike of the student and practitioner, those practical observations and rules for treatment, which his honourable and useful position has enabled him to acquire.

*Fifth Annual Report of the Sanitary Commissioner with the Government of India.* Calcutta: 1869. Office of Superintendent of Government Printing.

The Fifth Annual Report of the Sanitary Commissioner with the Government of Bengal (Dr J. M. Cunningham) has just reached us, and we propose devoting a short space to the diseases from which the European army there has chiefly suffered during the year 1868, making extracts, when necessary, from the compendious tables by Dr James L. Bryden, which are annexed. Before, however, doing this, we shall consider the circumstances in which the soldier lands in India, his stay at the depôt, and progress up-country; the state of barracks, and general conservancy of stations—all of which have great influence on health; and we shall, in this portion of our paper, draw a good deal on our own experience of times now past. The soldier, in the present day, ought to be landed in India in perfect health. Carefully examined before enlistment; sent out by the Red Sea during the best season of the year in magnificent Government steam-transports, or round the Cape in the finest sailing-vessels, also timed to arrive in the best season; supplied with excellent food, antiscorbutics, and even with preserved meat and vegetables. We are surprised to find that this is not always the case, for Dr Pain of the 11th Hussars states that his regiment suffered from scurvy from long confinement on board, and ill-ventilated and crowded transport. This is considered to have brought on a state of defective health, which, coupled with fatigue and exposure on the march to Mhow, led to an outbreak of typhoid fever at that station. As soon as the soldier is landed in Bengal, he is at once transferred to the depôt at Chinsurah, and thence forwarded up-country by rail. It is essential that this be done without delay, as the men are thus removed to a better climate, and carried away from the temptations by which they are beset without having the advantage of regular regimental discipline to check them. If a short delay even during the cold weather is attended with risk, a detention during the hot and rainy season is a most serious thing. In 1868, in consequence of late arrival, about 300 recruits remained at Chinsurah during the above-named season, and the result was that an average strength of 275 men lost 21, chiefly from cholera. On the other side of India, at Kurrachee (on the mouth of the Indus), the same thing occurred from the same cause, and 9 men, all under 24 years of age, were carried off by typhoid fever.

The journey up-country is now easily accomplished. In the old days two routes were open—one by the Ganges, involving, before steamers were introduced, months of tedious river work in crowded country boats, with no little risk of wreck during the rains and detention in the cold weather, from low water and contrary winds. After the introduction of steamers the passage was more rapid, b the flats were often greatly crowded, cholera was no very uncommon visitant, and it generally took from twenty-five to thirty days to reach Allahabad, at the head of the navigation, and only 600 miles by water from Calcutta. The other route was by the Grand Trunk Road, in which case the men marched from twelve to sixteen miles daily, carrying tents, etc., along with them, encamping on spots set apart for the purpose, where, on arriving in the morning, the rations for the day were found ready for the cooks. This was pleasant, but after a time apt to get tiresome; some of the encamping-grounds got infected with cholera, and severe fevers were occasionally caught in passing this, the more jungly district; and if the march were a long one, either the start was made before the cold weather had properly set in in Bengal, or, if not so, it was prolonged into the hot season—both injurious to health. All is different now: the men are placed in railway-carriages, are conveyed a moderate distance (I think about 150 miles), stop at a temporary depôt where every arrangement has been made for food and rest, and are passed on next morning; a couple of days' railway travelling thus enabling them to cover the ground which it would have taken them a month to march over. On the Indus, the troops have still to embark in flats (large floating barracks), towed by steamers for a part of the journey up the river to the Punjab; but the wanting link is being completed, and, in a year or two more, every man will be conveyed to the North-West Provinces or the Punjab with the maximum of speed and minimum of exposure.

The soldier, when arrived at his station, has to be housed. The barracks have of late been very greatly improved; and, whether we look to cubical contents, area per man, or any other circumstances, are an immense change for the better on the old ones—many of these not over well ventilated, generally greatly overcrowded, with the verandahs filled by the married men and their families; and in those days neither punkahs[1] nor tatties[2] were allowed. Attention has, it is stated, been of late paid to the water-supply, and an analysis has been made of it at several places. It is said to be, as a rule, originally good, and, where inferior, to have been tainted by deficient conservancy; this is undoubtedly true, and we trust that strict attention will be paid to this subject. We remember stations of old, with the wells in close proximity to cesspools, and assuredly much sickness used to be caused by this, at that time, unsuspected cause; of late dry-earth conservancy has been adopted in several regiments, has been approved of, and will be universally enforced. This system is well suited to India, and is in most stations essentially requisite to prevent the fouling of the water-supply —the soil being often porous, even sandy; and the contamination of wells from cesspools being merely a question of time—a certainty to occur sooner or later—stringent orders have been issued to preserve the surface of the soil in the neighbourhood of barracks in a state of purity; and a filter, invented by Dr Macnamara (a zinc vessel, containing a layer of animal charcoal and sand), has been approved of, and, if it continues to work well, will be generally introduced. The Punjab Government has recommended a method of storing cool water, said to be commonly practised in Persia and Afghanistan, which, if it be found to suit in India, will be no small boon.

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[1] Punkahs: large fans for moving the air in the barracks.

[2] Tatties: screens of Kuskus grass, which are put up against an open door, kept watered, and the dry hot west wind passing through the interstices absorbs moisture, and enters the apartment greatly cooled.

The Report makes mention of the food-supply in one instance only, stating that the cystic disease in cattle has been so prevalent in the Punjab that, during 1868, "1038" head of cattle—equal to 6·12 per cent. of those tendered for rations—have been found infected. Until lately these were rejected, and the meat destroyed; but this course being found to involve great loss to the State, and difficulty in keeping up the meat-supply, orders have been modified (it is not stated how); and it is hoped that, with the precautions now enjoined, the risk of tapeworm will be obviated. In most, if not all stations, regimental gardens have been established, and attended with much success, proving a healthy and invigorating employment to the men, and giving an increased supply of excellent vegetables for food. Regimental workshops, also, are now almost universal, and are an important means of not only banishing ennui, but of giving a good money return to the men who spend their time in them. Gymnasia are being constructed. Experimental in four or more of the largest stations, these are specially constructed, roomy and thoroughly fitted up; and *barrack* twenty to twenty-five in the ground floors of the new two-storied barracks. The exercises practised in them tend to develop the systems of the soldier; and, besides being useful at other seasons, must be specially so during the hot season and rains, when so little out-of-door exercise can be taken. Running-drill in the infantry, and the practice of fencing, single-stick, etc., in the cavalry (mounted and dismounted), will also be useful. We have now placed our men in barracks, let us consider the diseases from which they suffer; but, as diseases vary greatly over so extensive a country as that included in the Report, we shall follow Dr Cunningham's divisions, which are indeed the natural ones: 1. Bengal, containing Fort-William, Dum-Dum, Barrackpore and Berhampore, and the Chumnah depôt. 2. Dinapore, containing Dinapore, Darjuling, Huzareebaugh, Benares, Fyzabad, Lucknow, Seetapore, Futtegurh, Cawnpore, and Allahabad. 3. Meerut, containing Meerut, Shahjehanpore, Bareilly, Nyneetal, Landour, Roorkee, Moradabad, Delhi, and Muttra. 4. Agra, containing Agra, Morar, Gwalior, Jeepore, Jhansi, Nowgong, Saugur, and Jubbulpore; and 5th, Punjab, occupying the whole of the country so named, and garrisoned by 12,576 Europeans, being the main body of the British army in the presidency. Of the diseases which carry off our men, hepatitis stands at the head, the mortality from this having been 3·42 per 1000 during 1868—a higher than that for 1866 or 1867, which was 2·71 and 2·57 respectively. This, in all probability, depended on the character of the season.

Admissions from Hepatitis during the Year 1869.

| | Jan. | Feb. | March. | April. | May. | June. | July. | Aug. | Sept. | Oct. | Nov. | Dec. | Total Admissions per cent. |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| Bengal, . . | 93 | 101 | 116 | 124 | 144 | 196 | 170 | 153 | 194 | 122 | 94 | 104 | 5·10 |
| Dinapore, . | 18 | 21 | 26 | 28 | 31 | 45 | 34 | 43 | 49 | 24 | 18 | 28 | 4·91 |
| Meerut, . . | 7 | 10 | 22 | 27 | 40 | 54 | 52 | 32 | 43 | 27 | 18 | 21 | 7·70 |
| Agra, . . | 22 | 24 | 23 | 15 | 23 | 19 | 22 | 14 | 20 | 21 | 17 | 18 | 5·71 |
| Punjab, . . | 27 | 25 | 26 | 40 | 41 | 64 | 48 | 45 | 69 | 80 | 32 | 18 | 3·70 |

Mortality from Hepatitis during the Year 1869.

| | Jan. | Feb. | March. | April. | May. | June. | July. | Aug. | Sept. | Oct. | Nov. | Dec. | Mortality per cent. |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| Bengal, . | 9 | 7 | 9 | 7 | 5 | 9 | 9 | 9 | 17 | 14 | 7 | 6 | 3·42 |
| Dinapore, . | 1 | 8 | 0 | 1 | 0 | 3 | 1 | 2 | 6 | 1 | 1 | 1 | 2·69 |
| Meerut, . | 2 | 1 | 2 | 3 | 1 | 2 | 3 | 5 | 5 | 5 | 2 | 2 | 7·20 |
| Agra, . . | 1 | 1 | 1 | 1 | 1 | 1 | 2 | 0 | 1 | 3 | 1 | 2 | 3·60 |
| Punjab, . | 2 | 1 | 2 | 1 | 3 | 3 | 2 | 0 | 4 | 2 | 2 | 0 | 1·83 |

I have, in the table given, shown the admissions and mortality, month by month, and also the ratio of each per cent. Both admissions and mortality have been much increased in the third group by the prevalence of the disease in the Nyneetal depôt. No explanation is given of this. The table shows a tolerably regular increase of admissions from January to June, corresponding with increasing temperature. July and August are generally more favourable, but September, which is usually close, hot, and damp, is again more unfavourable. October and November show progressive decrease, followed in December by an almost universal increase, probably the result of the increased exposure, which all consider themselves entitled to risk at that season. The death table is somewhat different: commencing with a rate in January (the legacy from the previous year) somewhat higher than subsequent months, after the next month or two the numbers increase, but not very regularly, until they culminate in September; and October is also a heavy month. November and December show gradual decrease.

Fevers show the next highest ratio of mortality for 1868, having been 2·88 per 1000. The admissions per cent. to strength, and the mortality per cent., are given below. From a table which is not given here, we find that in every circle the month of June stands first, both for numbers of admissions and deaths. In the Punjab, May has the next greatest number of admissions, and July of deaths; in the other circles September is on the whole the worst. When we consider the proportion of sick to strength per cent. of *all* fevers, the Punjab stands first, giving 65·52; next Agra, 53·38;

then Bengal, 46·24; Dinapore and Meerut being nearly alike, viz., 32·12 and 32·23; and when we look at the proportion of continued and remittent fever, we find the Punjab still in the van, 19·67; then Bengal, 14·15; Meerut, 11·89; Agra, 7·65; and Dinapore, 6·86. The deaths from intermittents are few, ranging from ·00 in Meerut to 0·15 per cent. on admissions in Dinapore; and the rate for continued and remittent ranges from 1·25 in Agra, up to 2·75 in Meerut. The number of cases of enteric or typhoid fever has been considerable. It is not easy to state how many, as this disease is probably frequently returned as remittent: of the deaths, however, no fewer than 35 (of a total of 91 from fevers) are recorded, in which the nature of the disease was verified by post-mortem, showing ulceration of Peyer's glands.

| Division. | Disease. | Per cent. of Admissions. | Per cent. of Deaths. |
|---|---|---|---|
| Bengal, . . . | Total intermittent, . . . . | 32·09 | ·07 |
| | Total remittent and continued, | 14·15 | 1·88 |
| Dinapore, . . . | Total intermittent, . . . . | 17·26 | ·15 |
| | Total remittent and continued, | 6·86 | 2·55 |
| Meerut, . . . | Total intermittent, . . . . | 20·34 | ·00 |
| | Total remittent and continued, | 11·89 | 2·75 |
| Agra, . . . . | Total intermittent, . . . . | 45·73 | ·06 |
| | Total remittent and continued, | 7·65 | 1·25 |
| Punjab, . . . | Total intermittent, . . . . | 41·85 | ·06 |
| | Total remittent and continued, | 19·67 | 1·58 |

Almost all the deaths occurred in young men—all during the first year of Indian service, and the great majority during the hot season. This would appear to point to defective drainage and polluted water-supply, the wells at this period of the year being always low, and the tendency to contamination from cesspools, or any reservoir of filth, being of course greater than at other seasons. We should have liked to see some allusion to irrigation as a cause of fever of the intermittent and remittent type. Moisture undoubtedly has great effect in producing such complaints, whether natural as in Bengal (where Berhampore is described as being a swamp, and is so), or artificial as from irrigation in the North-West Provinces and Punjab. It is somewhat singular that it is irrigation from canals which is injurious, that from wells being as a general rule harmless; and it has occurred, that the introduction of water by a canal has greatly affected the health of a neighbourhood. Have the Europeans suffered from this?

Moisture is not, however, always necessary for the production of fever; the most deadly season in the Terrai is the months of May and June, at which time not only is the surface thoroughly dried up, but no water is to be found unless at excessive depths. It is curious that, after exposure to malaria in such places, the longer the fever incubates, the greater is the danger—a day or two producing intermittent, a week serious remittent, and fifteen days, a certainly fatal fever of that character.

| | Jan. | | Feb. | | Mar. | | April. | | May. | | June. | | July. | | Aug. | | Sept. | | Oct. | | Nov. | | Dec. | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | Admit. | Died. | Admit. | Died. | Admit. | Died. | Admit. | Died. | Admit. | Died. | Admit. | Died. | Admit. | Died. | Admit. | Died. | Admit. | Died. | Admit. | Died. | Admit. | Died. | Admit. | Died. |
| Bengal, . | 1 | 1 | 2 | 0 | 1 | 0 | 2 | 2 | 5 | 4 | 61 | 32 | 39 | 17 | 114 | 24 | 13 | 8 | 2 | 0 | 0 | 0 | 1 | 0 |
| Dinapore, | 0 | 0 | 0 | 0 | 0 | 0 | 1 | 1 | 2 | 1 | 23 | 15 | 14 | 6 | 2 | 3 | 3 | 2 | 0 | 0 | 0 | 0 | 0 | 0 |
| Meerut, | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 1 | 0 | 3 | 2 | 12 | 6 | 5 | 2 | 1 | 1 | 0 | 0 | 0 | 0 | 0 | 0 |
| Agra, . | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 6 | 6 | 4 | 1 | 2 | 0 | 4 | 8 | 0 | 0 | 0 | 0 | 1 | 0 |
| Punjab, | 1 | 1 | 0 | 0 | 0 | 0 | 0 | 0 | 1 | 1 | 22 | 8 | 9 | 4 | 105 | 19 | 5 | 2 | 1 | 0 | 0 | 0 | 0 | 0 |

Apoplexy, probably invariably heat apoplexy, has caused the next highest mortality. From the table given above, it will be at once apparent that June, July, and August are the months in which this disease is most prevalent and most fatal. It must not be supposed that direct insolation is required to produce the complaint, many men are taken ill in barracks—even during the night, especially just before the great outbreak of the rains, when the atmosphere is charged with electricity, the heat is intense and suffocating, and but little relief to be obtained from tatties and punkahs, there being generally almost no wind, and what there is, loaded with moisture. The same character of weather, after a lull in the rains, or at their termination, is generally productive of a fresh lot of cases. Observe the small mortality noted in the Punjab division in the month of September: of these, it is stated that 91 occurred in troops marching to the frontier camp; although the number struck down was large, and the exposure to heat must have been great, yet the result is favourable.

Cholera carried off 1·81 per thousand of our soldiers during the year, although no cases occurred in the Punjab, and only two (neither fatal) in the Meerut circle. The table given here shows at a

| | Jan. | | Feb. | | Mar. | | April. | | May. | | June. | | July. | | Aug. | | Sept. | | Oct. | | Nov. | | Dec. | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | Admit. | Died. | Admit. | Died. | Admit. | Died. | Admit. | Died. | Admit. | Died. | Admit. | Died. | Admit. | Died. | Admit. | Died. | Admit. | Died. | Admit. | Died. | Admit. | Died. | Admit. | Died. |
| Bengal, . | 0 | 0 | 1 | 1 | 6 | 4 | 10 | 6 | 10 | 10 | 20 | 13 | 8 | 5 | 16 | 6 | 5 | 4 | 3 | 2 | 4 | 3 | 4 | 3 |
| Dinapore, | 0 | 0 | 0 | 0 | 0 | 0 | 1 | 1 | 0 | 0 | 1 | 0 | 0 | 0 | 11 | 5 | 4 | 4 | 0 | 0 | 0 | 0 | 1 | 1 |
| Meerut, | 0 | 0 | 0 | 0 | 0 | 0 | 1 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 1 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 |
| Agra, . | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 3 | 3 | 10 | 7 | 3 | 2 | 4 | 1 | 1 | 0 | 2 | 1 | 0 | 0 | 1 | 0 |
| Punjab, | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 |

glance the number of admissions and of fatal cases for each month in each circle, the mortality per thousand running from 1·48 in Dinapore, up to 3·36 in Agra. When we regard the proportion of deaths to admissions, we find that they were in Agra, 38·33; in Dinapore, 61·11; and in Bengal, 65·52 per cent.; and that the greatest number of admissions occurred in August (32), and next in June (31). It is most usual for ordinary cholera to appear about or during the rains, as during the year under consideration; but a pure epidemic may, and often does, occur at other times. We have known this to happen as early as April, and as late as October, while cases may occur at any time; indeed, it will be observed that the month of January is the only one in which no admission is recorded from this disease. It is by no means satisfactory to observe the mortality, which appears of late years to have increased. During the early outbreaks of the disease, one-third of deaths was pretty nearly the accepted ratio; and it was alleged that it mattered little what treatment was followed, the ratio in the end would be much the same.[1] The disease is alleged to have been imported into Jubbulpore, but it is said that facts have not been cited to support the statement. During a pretty extensive service in India, we have seen much cholera, and, with one exception, we never saw anything leading us to suppose that the disease was contagious. What we have seen, however, in this country, has caused us to alter our opinion, and to allow that cholera must be communicable; at the same time, experience appears to point to the conclusion, that unless the conservancy of a place be bad, and specially the water-supply contaminated, the disease, although introduced, will not become epidemic, but will die out like a fire for want of fuel. Allusion is made to the special investigation now being carried on as to the alleged fungoid nature of cholera; and it is stated, that so far as they have gone, the theory appears to be incorrect. The state of the subsoil as to water is also to be investigated. With regard to this branch of the subject, we can state that we have known outbreaks of cholera to occur before the setting in of the rains, when there was not a drop of water in the station nearer the surface than from 25 to 35 feet. We would also remark, that we have known cholera to break out in two nights over a long range of country; as on the 25th and 26th of May 1860, when, from Rajmahal to Mirzapore inclusive, cases occurred in nearly every principal town or station along the Ganges. We have also known it to creep at a singularly slow rate, even along a well-travelled road, taking something between twenty and thirty days to cover forty miles. The inquiry is a most interesting one; and we shall look anxiously for further information.

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[1] No mention is made of the treatment followed during the year. We have seen in this country very promising results from doses of strychnine ($\frac{1}{8}$th of a grain every two hours), until the characteristic symptoms of the drug were produced; moderate allowance of soda-water for drink, unlimited ice to suck, and no stimulants during the early part of the treatment. Of course the nursing must be efficient.

*Dysentery and Diarrhœa.*—Without constructing tables, it may be enough to state, that 7·56 of every hundred deaths are due to the first of these diseases, and 0·94 to the latter. The months in which they prevail are generally June to August inclusive, in the Agra and Punjab circles extending to September, and in Meerut to October: the admissions per cent. of strength for the year vary from 9·48 in Meerut to 11·49 in Agra.

Omitting injuries and deaths out of hospital, phthisis, heart and respiratory disease, delirium tremens, atrophia, and aneurism, as these can scarcely be called Indian complaints, we shall close with a few words on venereal, not a specially Indian complaint, but one which everywhere demands our serious attention, proving, as it does, such a source of inefficiency. With the view of reducing this, lock hospitals have been established, the bazaar women registered, and arrangements made for their regular inspection. The result for 1868 has not been satisfactory; for while in some stations there has been a decrease, in more the reverse has been the case. It would appear that many causes have contributed to this—irregular attendance on inspections, imperfect registration, and the small number of those treated who must have been spreading contagion. It is evident that if inspection is to be performed at all, and treatment undertaken, both must be fully and strictly carried out, otherwise failure is certain.

It will appear from the remarks and tables above, that, as regards diseases in our Indian army, the months of June, July, August, and September give the greatest number of admissions and deaths. During these months the soldier is generally confined to barracks, except for a short time in the early morning and after sunset; and there can be no doubt that the improvement in the health which has of late years taken place, is greatly to be traced to improvements which have been more or less alluded to,—the lofty barracks, free from overcrowding, punkahs, tatties, good and more varied food, workshops, libraries, swimming-baths, malt liquor (instead of arrack or rum), improved conservancy, a more sensible style of dress;—each and all combine to make the soldier more comfortable, more healthy, and more happy. Could we only select our stations irrespective of strategic reasons, we should be able to have a still greater improvement; but in many instances large cities and masses of population compel the placing of troops in localities which are very objectionable. Even in such situations, however, great strides have of late years been made in the right direction; and we can point to stations which in old times gave a regular mortality of ten, now reduced to little more than two per cent. per annum. Is there not, then, good reason to thank God, and take courage?

*Practical Anatomy: A Manual of Dissections.* By CHRISTOPHER HEATH, F.R.C.S., Assistant-Surgeon to University College Hospital, and Teacher of Operative Surgery in University College. London: Churchill & Sons: 1869. Pp. 596. 2d Edition.

*Human Osteology.* By LUTHER HOLDEN, F.R.C.S., Surgeon to, and Lecturer on, Anatomy at St Bartholomew's Hospital, etc., etc. London: Churchill & Sons: 1869. Pp. 283. 4th Edition.

*Descriptive Anatomy of the Horse and Domestic Animals.* Chiefly compiled from the Manuscripts of THOMAS STRANGEWAYS, LL.D., M.R.C.V.S., late Professor of Anatomy in the Veterinary College, Edinburgh; and of the late PROFESSOR GOODSIR, F.R.SS. L. and E. By J. WILSON JOHNSTON, M.D., F.R.S.E., Member of the Royal College of Veterinary Surgeons, Bengal Medical Staff; and T. J. CALL, L.R.C.P.E., Honorary Medical Officer to Ilkley Hospital. Edinburgh: Maclachlan & Stewart: 1870. Pp. 525.

ON the re-appearance of Heath's Manual, as a second edition, we have only to congratulate the student on the improved accuracy of this admirable manual, which is as concise as any student (*sic*) could wish, and yet accurate enough to satisfy the modern-minded examiner. Useful as it may be made as a grinding book, it is mainly intended, as its title indeed implies, as an aid to dissection, for which it is admirably adapted.

The fourth edition of Mr Holden's well-known work requires no recommendation from us. During its well-deserved successful career it has undergone successive improvements, and in this edition all the lithographic illustrations have been re-drawn, so that in every respect it is superior to all the previous issues of the work.

The last work, however, on the present list, Strangeways's "Descriptive Anatomy of the Horse and Domestic Animals," deserves to be dwelt on a little longer. Those who are acquainted with the struggles of veterinary science in this country since the days of Blaine's admirable but too concise work on the veterinary art, and who remember the delight with which Percivall's "Anatomy of the Horse" was hailed, cannot fail to acknowledge, as a further progress in the right direction, a work which includes not only the anatomy of the horse, but also that of all the domestic animals—the ox, sheep, pig, dog, and cat; animals whose anatomy has hitherto been ignored, except in so far as displayed by the unscientific knife of the butcher, or the partial researches of the practical physiologist. We hold that veterinary surgery is the most important science of our day, because upon it depends not only the material prosperity, but the very existence of our race in its present condition of advanced civilisation. And popular opinion bears out our views. Look at the palatial stables built for our racers, hunters—nay, for our very farm-horses, and contrast them with the hovels on the same estate in which the human labourers live. Hear what suffering humanity itself says: A grieve in Fifeshire complained that his doctor charged him half-a-crown a visit, which he thought was too much, adding querulously, that he could have got a cow-doctor for as much. All honour to our grieve's high appreciation of the value of veterinary science, which seemed so superior—even in its elementary branches—to his own pressing requirements.

The food question, too, is daily forcing upon the public the importance of veterinary science. And yet, strange to say, the very man to whom we owe the preservation of our flocks and herds from threatened extinction—the man whose counsels, compressed into less than two years, and who, had his advice been followed, would have annihilated in two months an incurable disease, which ravaged our herds last century for ten years—is now a wanderer far from the place where his advice was given, because his fellow-countrymen have failed to recognise and to reward personal talent and perseverance against prejudice and vested interests, which have culminated in a national success. It may seem an insult to name this gentleman to those who are acquainted with the history of the cattle-plague in Britain, and yet it is due to himself to say, that to the advice of Professor John Gamgee, too tardily acted upon, we owe the preservation of our present stock of animal food. And we may add, that we hope it is not yet too late to reward services which have proved to be of incalculable value to all, both food-producers and food-consumers.

So far as the text of this work is concerned, it is admirable, concise, a little bald, but correct; but the plates are scarcely distinct enough, and are still more marred by having several wrong references supplied, so that for a student they are useless, because, of course, where even one is wrong, he cannot depend upon any being right. In Dr Johnston's *memoria technica*, also, in relation to the dentition, it is somewhat absurd to find it stated that the incisors of the ox and the sheep are to be found in the *upper* jaw only, when every herd lassie knows that it is exactly the reverse. With these exceptions, however, which scarcely detract from the merits of a work which is unique in the English language, and which can be readily remedied in a second edition, we cordially recommend this work to the student of veterinary or comparative anatomy; and we are glad to understand that it has been adopted as a text-book by the Veterinary Colleges, both of Glasgow and Edinburgh.

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*The Medical Directory for* 1870. London: Churchill & Sons.

THIS indispensable volume continues to maintain its high character for fulness and accuracy. It is, we consider, much improved by the adoption of the suggestions we threw out last year, and the consequent change of title.

*The Neuroses of the Skin: Their Pathology and Treatment.* By HOWARD F. DAMON, A.M., M.D. Philadelphia: Lippincott & Co., 1868: Trübner & Co., London.

AMERICA seems in no way disposed to lag behind Europe in its newly-developed zeal for the study of dermatology, but, like all that the people of that great country undertake, is determined not only to equal it, but in the future to take the lead in the study of this department of medical science. Thus, we read in a letter from Dr Weisse of New York to Mr Erasmus Wilson, published in the October number of the *Journal of Cutaneous Medicine*, that since they awakened to the importance of the study of skin diseases, the leading medical colleges in the United States have established chairs for the teaching of this speciality. There "weekly clinics are given before the class by the incumbent lecturer or professor. Besides these college clinics, of which there are two a week, each of the ten dispensaries in the city of New York has a department for the treatment of diseases of the skin, where patients are treated daily." In addition to this, a New York Dermatological Society has been organized, consisting of twenty-five resident members to commence with. The main feature of the meetings of this society is to be the giving of "clinical histories of cases and the presentation of patients," besides affording to all interested in this department of medicine "an opportunity for a free exchange of their opinions and methods of practice, and to contribute American experience and investigation to the fast-accumulating stock of knowledge of the diseases of the skin." Such are the aims and objects of the Dermatological Society of New York. And, with the well-known energy and acuteness of Americans, it is only to be expected that this band of twenty-five earnest workers will soon lay before the world some new facts in the more recondite departments of dermatology.

We have been led to these remarks by seeing before us a book on this subject from the pen of one of those workers, and which is probably one of the first on the subject produced in America. It is entitled "The Neuroses of the Skin—their Pathology and Treatment," and merely takes up the first division of the author's classification. For, like a true American, he cannot rest content with any one of the many various systems that have already been proposed in the Old World, but promulgates one of his own. He maintains that "all skin-diseases may be separately included under one or more of the following lesions:—Sensibility, secretion, nutrition, and structure. Each of these lesions in our system of classification constitutes a natural division or class," to wit:—

Class I. Neuroses of the Skin.
„ II. Functional Diseases of the Cutaneous Glands.
„ III. Inflammations of the Skin.
„ IV. Structural Lesions of the Skin.

We cannot but regret that in his opening sentences the author, in introducing his own scheme to the medical world, should have thought fit to disparage, and somewhat flippantly criticise, that propounded by so great a master as Hebra. So far from exalting himself, or his system, by such unworthy means in the eyes of his readers, at least in this country, such criticism recoils merely upon his own head, and disposes one to receive what he has to say with much less favour than might otherwise have been the case.

Under the head of Neuroses we find that Dr Damon treats of two kinds; viz., those which are caused by peculiar conditions of the sensitive nerves, and of the vaso-motor nerves, exhibited in the former case either as hyperæsthesia or anæsthesia, and in the latter as spasm or paralysis. And under the title of Hyperæsthesia are grouped the four following affections:—Dermalgia, Prurigo, Urticaria, and Zoster.

Nothing more is needed, in our opinion, than the mere enumeration of these affections, to show the fallacy of this "new system of classification of skin-diseases." The idea of taking one symptom which happens to be common to a number of utterly different disorders, and so grouping them together into one class, irrespective of the cause and origin of this one particular symptom, is wholly untenable. The origin of urticaria, for instance, as Dr Damon himself points out, is to be sought for in those articles of diet which produce more or less disturbance of the digestive organs. Here the cutaneous disturbance is altogether a secondary condition, a reflex phenomenon, and not the result of disease in the nerves themselves. Is there no nervous disturbance, we would ask Dr Damon, in the inflammations of the skin, no Dermalgia, as he terms it, no pain? As well may inflammations of the skin be included under the neuroses of the skin as urticaria. The affections in which the nerve-disorder is the first original cause are very few and very rare.

The various diseases taken up are well handled, and the treatment fully, and apparently judiciously, gone into. The book is well got up and neatly printed.

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*Vaccination: A Condensed Summary of the Evidence in its Favour, and of the Objections urged against it.* By J. THORBURN, M.D. London: Simpkin, Marshall, & Co. Manchester: J. E. Cornish: 1870. Pp. 59.

IN the present day, when most of the old beliefs are being questioned, we cannot wonder that in our profession certain theories and methods of treating various diseases have been called in question. But we surely might have expected that such a boon as vaccination would have been exempt from censure and opposition. But such is not the case. There have always been a few disbelievers in vaccination, and since the passing of the Compulsory Vaccination Act, the spirit of antagonism has been fomented, an Anti-Vaccination League formed, and strenuous exertions made to excite the people against that law, and the practice it enjoins. Ignorance is, without doubt, the chief cause of this hostility, and with the laudable object of giving correct information on the question, Dr Thorburn has written the above pamphlet. In it he briefly and clearly traces an outline of the subject under the following heads:—Smallpox, Inoculation, Vaccination, Results of Vaccination, Drawbacks to Vaccination, Objections to Vaccination, and Compulsory Vaccination.

In moderate space, Dr Thorburn has placed within reach of the non-professional reader the principal facts, statistics, and arguments relating to the topics he discusses. But while sensible of the substantial merits of the pamphlet, we must express regret that more care has not been bestowed on the language, which is occasionally open to criticism, and would be much improved by a little pruning. Moreover, the sarcastic humour with which his adversaries are handled might advantageously have been held in check. Doubtless, it is difficult to deal with unfounded assertions and reckless misrepresentations, and at the same time preserve the due calmness and gravity; but ridicule, though it may put an opponent to shame, perhaps to silence, scarcely ever can advance a benevolent cause. A different tone and method of treatment must be adopted, if we desire to convince the judgment and persuade the will, to disarm hostility, overcome opposition, and secure cordial co-operation; and surely these, in cases like the present, are the results at which the philanthropist should aim.

*A Manual of Diet for the Invalid and Dyspeptic.* By DUNCAN TURNER, L.R.C.P.L., L.R.C.S.E. London: Churchill & Sons: 1869. Pp. 72.

THE formulæ in this book, which are what one chiefly looks at, are in no respect different from those to be found in any decent cookery book; and many, if not most, of them might be very readily improved by a little practical experience. The other parts of the book, especially the remarks on nursing, contain nothing new, the latter being chiefly borrowed from Miss Nightingale's admirable brochure. We decidedly object to the Highlands of Scotland and those of Switzerland being at all placed on a par in regard to salubrity. Change of air, within certain limits, may be beneficial at any season. Change of air to a different and distant parallel of latitude requires very careful consideration, especially as regards season. The Highlands of Scotland are the most salubrious part of Europe in autumn (quoted from an unpublished paper by Dr Lombard of Genoa); no data, however, exist to show that the Highlands of Switzerland are equally salubrious at any season. This brochure is, however, though full of platitudes, one that may be very useful to those who have no access to better information.

*The Animal World: A Monthly Advocate of Humanity.* London: Partridge & Co.

A MONTHLY paper published for the advocation of humanity to all classes of animals: it is beautifully illustrated, containing many charming tales, besides a great deal of valuable information bearing upon its own speciality. We cordially recommend it to our readers, many of whom take a loving interest in all animals, from man downwards.

# Part Third.

## MEETINGS OF SOCIETIES.

### MEDICO-CHIRURGICAL SOCIETY OF EDINBURGH.

### SESSION XLIX.—MEETING III.

*Wednesday*, 15*th December* 1869.—Dr BENNETT, *President*, in the Chair.

*Dr Bennett*, on taking the Chair, returned thanks for the honour which the Society had done him in electing him President. He also stated that, while he believed the combined work of a society to be necessary, and to be the best way of advancing medical science, he conceived that the present mode of conducting the business of most medical societies was not that most conducive to the end desired. The thoughts of individual members upon particular topics were either crude and possessed of no novelty or interest for the senior members; or, if delivered by these seniors, as the result of long and successful inquiry, they were so exhaustive and complete as to leave little room for discussion. A brilliant debate in such a society was, therefore, only synonymous with criticizing a subject regarding which nothing certain was known; and regarding which there was, therefore, room for the greatest diversity of opinion. Such a display of polemical medicine, however conducive to personal animosities, was not the way to advance medical science; an object which could be only surely gained, like the taking of a fort, by the combined effort of many men slowly working towards a determined [illegible] Dr Bennett pointed out the enormous waste of labour—especially in relation to therapeutics—continually taking place in our hospitals; and illustrated the difficulty of obtaining combination among medical men, by a reference to the trifling results arrived at by the British Medical Association to obtain information, through special committees, in regard to the treatment of certain diseases. Six or eight returns only—and in no case more than seventy cases—were obtained from more than three thousand members! After thus pointing out the impossibility of getting combined work out of the profession generally, Dr Bennett stated that the only possible way of getting profitable results was, in his opinion, to select a committee of men distinguished for special knowledge in separate branches of medical science and practice, and to combine them in the working out of some settled problem. For that end money was no doubt required; for, in our laborious and over-worked profession, it was in vain to expect, and wrong to ask, any one to labour for nothing; and, besides, the necessary expenses of the inquiry would have to be provided for. He believed that, with our present advanced knowledge and with the instruments of precision we now possessed, there were few problems in medical science incapable of being accurately solved. He therefore thought that it ought to be an object of ambition with every medical society to aid in such inquiries; and he was sure the funds of those which had them to spare could not be devoted to a more philanthropic use, or one more likely to conduce to the objects they were founded to promote.

I. *Dr Watson* then showed a TESTICLE which he had removed at the hospital to-day on account of tubercular disease of the organ. The disease commenced more than a year ago in conseqnence of a blow received upon the part. More recently an abscess formed, and had been evacuated by incision before admission to hospital. The extent of the disease, the complete implication of the organ, and its consequent disorganized state, rendered excision of the diseased parts the only means of relieving the patient.

II. THE RIB OF A RABBIT, which he had recently extracted from the rectum of a patient suffering from fistula in ano. The sharp extremity of the rib projected beyond the walls of the rectum into an abscess sac, which had already opened upon the surface of the skin close to the anterior aspect of the anal orifice. The case was interesting as illustrating the fact that abscess in the ischio-rectal fossa may be caused by the bone of a small animal becoming impacted in the rectum, and that the abscess may secondarily open upon the surface, constituting a complete anal fistula, of which the internal opening caused by penetration of the rectal walls from within constituted the first step in the process—a mode of formation which has been denied by some writers. This patient stated, that three days before the painful symptoms commenced, he had dined off rabbit.

III. THE DETRITUS OF A SMALL OXALATE OF LIME CALCULUS, which he had yesterday removed within the blades of a scoop-lithotrite from a patient who had suffered from urinary symptoms for three months. The case had been attended at first by symptoms of difficulty in micturition and actual retention. The difficulties in micturition had, before coming under Dr Watson's care, been treated by the introduction of flexible bougies. On introducing the scoop-lithotrite the calculus was at once seized, crushed, and withdrawn from the bladder by means of the instrument.

IV. *Mr Annandale* showed A NEW SPLINT which he had lately devised for the treatment of cases of excision of the knee-joint. The advantages of the splint were that it could be applied immediately after the operation, and admitted of being shortened or lengthened with ease. In addition, it was light, cheap, and could be readily swung in a Salter's cradle.

V. A PREPARATION, showing the result of an operation for ligature of the femoral artery. The ligature employed was antiseptically prepared catgut. The patient died on the fifth day from heart-disease, and an examination of the wound determined an absence of all suppuration, and a perfectly secure condition of the ligature, which had undergone not the slightest change.

VI. A HIP-JOINT, illustrating the results of a recent dislocation of the head of the femur into the foramen ovale.

VII. TWO EXAMPLES OF SCIRRHUS of the female breast, which he had recently removed by operation. One of the cases was interesting from the fact of the cancer being in a condition of inflammation and suppuration.

VIII. THE ARTICULAR SURFACES of a knee-joint, which he had removed in a case of excision. The specimens well illustrated the condition of the cartilage described by Redfern and Goodsir.

IX. A CASE OF FRACTURED PATELLA in which a very long fibrous union had taken place.

X. *Mr Annandale* then read his case of UNCLOSED URACHUS, with umbilical fistula, which appears in our present Number, at p. 680.

*Dr Matthews Duncan* remarked upon the interest of the subject referred to, and inquired if any of the members were acquainted with a similar malformation of the omphalo-mesaraic or vitelline duct; part of this was well known to be frequently found remaining as an intestinal diverticulum, but he was not aware that it had ever been seen permanently open.

XI. *Dr Matthews Duncan* then read Dr Whiteford's paper on THE REMOVAL OF AN EMBEDDED UTERINE FIBROID, which appears in our present Number, at p. 697.

*Dr Joseph Bell* remarked, that he had read in a French medical journal that injection of the perchloride of iron as a hæmostatic had been productive of great pain, of sloughing of the mucous membrane, and subsequent adherence of the opposite surfaces of the vagina.

*Dr Bennett* remarked, that it seemed to him somewhat singular that when the patient was faint and weak, then tincture of digitalis was administered. He was aware that a modern idea was afloat that digitalis was a stimulant to the heart; this he conceived to be erroneous. He held to the ancient belief, that digitalis was a pure sedative to the heart's action, dangerous in cases of debility, and not to be continued any length of time, in sensible doses, without extreme caution. He referred to a case that occurred in his clinical ward last summer, in which there was great cardiac action with angina, and where infusion of digitalis was cautiously given and carefully watched. The result of repeated trials was to diminish the force, and render irregular the cardiac pulsations, without relieving the paroxysms of pain. Exact sphygmographic tracings of the pulse were taken before, during, and after the action of the drug, which showed the most marked diminution in the strength of the cardiac contractions, combined with alterations in rhythm. No stimulating action of the drug was at any time observed, though specially looked for. He thought that this was a subject which ought to be inquired into, and he believed it was one which could be readily and accurately settled.

*Dr George W. Balfour* said that he would be happy to assist in any such inquiry; that, though originally holding views similar to those of the President, his experience had taught him that those must be considerably modified. *First*, He had found that even in cases of mitral disease, which were peculiarly susceptible to the beneficial action of diagitalis, the improvement in the strength of the pulse was certainly not always nor necessarily concurrent with a diminution in its frequency. *Second*, In cases of aortic regurgitation, in which digitalis was ordinarily forbidden, from the supposed ill effects of slowing the cardiac action, in such cases he had frequently found the greatest benefit from its administration, not because the heart's action was slowed, but because it was strengthened and steadied—the very result we sought to attain in such cases; while, if it was slowed at all, it was only slowed by being steadied. *Third*, That the great danger of digitalis seemed to arise from idiosyncrasy, that is, from susceptibility to its action; that he had administered tincture of digitalis continuously for a period of two years with no result beyond continuous benefit, and yet he had seen the same dose of a similar preparation give rise to poisonous symptoms within a few days. Properly administered, digitalis seemed to be an admirable cardiac stimulant; improperly administered, a most unsafe and uncertain sedative; and our great object ought to be to ascertain the circumstances conducive to the development of the one action, and to avoid those which led to the other.

*Dr Matthews Duncan* replied, that he had never heard of any such case as that referred to by Dr Bell; that digitalis had been recommended as a hæmostatic in menorrhagia, and might have acted so in this case; that it was also a good diuretic, and such an action must have been beneficial. He also stated that enucleation of uterine fibroids was an old and frequently-proposed operation, always given up on account of its danger; but that nowadays it seemed fairly established, and was rarely, if ever, fatal; the great improvement consisting in dissecting the tumour *in situ*, and pulling it down, pushing back the uterus, if it should happen to get inverted. He had removed such a tumour weighing two pounds and a half, in which no cutting was required, the great difficulty being to get it through the *ostium vaginæ.* He believed this was the largest tumour of this kind ever removed in this country.

XII. *Dr John Duncan* then read his paper on GALVANO-PUNCTURE OF NÆVUS, wherein he advocated this operation. This paper will appear in our next Number.

*Dr Joseph Bell* related a case of cirsoid aneurism of the face, in which this treatment had been productive of most gratifying results.

*Mr Annandale* also made remarks upon the subject.

*Dr Duncan*, in answer to inquiries, stated that the application was only painful at the opening or closing of the circuit, and that from four to six pairs of plates were all that were required; but, from the aforesaid circumstance, these must be employed continuously throughout the operation.

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## OBSTETRICAL SOCIETY OF EDINBURGH.

### SESSION XXIX.—MEETING II.

*24th November* 1869.—Dr BURN, *President*, and afterwards Dr BELL, in the Chair.

The following gentlemen were elected office-bearers of the Society for the next two years:—

*President.*—Dr Charles Bell.
*Vice-Presidents.*—Drs Menzies and L. R. Thomson.
*Treasurer.*—Dr James Young.
*Secretaries.*—Dr R. Peel Ritchie and Mr G. Stevenson Smith.
*Members of Council.*—Drs Duncan, Cochrane, and Sidey.

Dr Johnston, Master of the Rotunda Hospital, Dublin, was elected an Honorary Fellow of the Society.

John Connel, M.B. and C.M., was elected an Ordinary Fellow.

## I. EXHIBITION OF DR ZIEGLER'S NEW MODELS AND CASTS. BY DR J. MATTHEWS DUNCAN.

The models and casts which are here shown were made by Dr Ziegler of Freiburg, under the direction of Professor Hegar. It was this latter gentleman who planned them. They are intended for the use of the student and of the teacher, and they appear to me to be likely to be of much service to science and to purposes of illustration in the class-room.

Much is already known of the mechanism of delivery, and the models are fitted to show this and to enable us to discover more.

Models of the kind now shown have been made by Fabbri of Bologna, with whose works I am quite unacquainted; by Hodge of Philadelphia, who dwells much on their value, and figures his own specimens in his great work entitled "The Principles and Practice of Obstetrics," published in 1864; and now by Professor Hegar.

It is only with the models of this last gentleman that I am actually acquainted, and there is, no doubt, great merit in them. They are a very praiseworthy achievement, and will be very useful. But we must not rest content with them, for there is still much to be expected from this kind of work. We need to have the homalographic system applied here, as it has been with much advantage in other departments of medicine. Sections of actual heads moulded under the influence of the violent pressure of a difficult labour should be made, as also homalographic sections of actual pelves with the heads in them, taken in cases of natural and in cases of difficult delivery. No doubt these will be produced in the course of time.

The present series consists of twenty-seven casts. These include models of the canal of the normal pelvis, sections of the canal, both oblique and direct, model sections of the fœtal head in many directions, and models of the canals of five kinds of abnormal or deformed pelvis.

The history of these models illustrates some interesting points; for instance, the difficulty of getting a good natural pelvis. To make his works, Hegar had to resort to Litzmann for a characteristic specimen of a female pelvis! He has adapted his models to the doctrine of the direct entrance of the fœtal head into the brim of the pelvis, not to that of Naegele. While this is, of course, satisfactory to me, I have, in conclusion, to say, that I look with anxiety for further researches in the mechanism of natural delivery, believing that our present views, while certainly rudimentary and imperfect, are also not without grievous flaws in the way of positive error.

*Dr Keiller* said that he quite agreed in the remarks made by Dr Duncan as to the necessity of studying the mechanism of labour more thoroughly than it had ever been done. Every practitioner ought to study the mechanism in every case, and in this way the obstructions which often took place during labour might be explained. Such a study tells us what no casts can ever do—it enables us to ascertain the influence of the various forces which are at work. The only objection to the casts shown, and indeed to all casts, was that they were too mechanical—they do not show the effects of pressure. He was of opinion that casts made of vulcanized india-rubber were required for this purpose.

*Dr Thomson* said that he had many times been called to cases where midwives had been long in attendance, and where a slight change of position effected a speedy delivery.

## II. A CASE OF PUERPERAL CONVULSIONS. BY DR ANGUS MACDONALD.

On 16th October 1869 I was summoned, about 4.45 o'clock A.M., to see Mrs B., aged 32, residing at No. 6 New Broughton. About a year previously she had had a miscarriage at the fifth month, for which she was attended by Dr Andrew Inglis, and made a good recovery, with no untoward symptoms. She was now about the full time of pregnancy, and daily expecting her confinement. For several days past, I learnt that she had suffered from severe headaches, but these had become much worse on the forenoon of the previous day (15th October). About one o'clock A.M. she fell asleep for about a quarter of an hour, and on awaking found that her sight was completely gone. She also felt sick, and vomited. About three o'clock she had a convulsive attack, which lasted about three minutes, and after an interval of half an hour this was succeeded by a second seizure, which continued for a little longer. At four o'clock she had another fit, after which her husband came for me. On my arrival I found the patient's face much flushed and puffy. Her legs, which I ascertained had been previously much swollen, were quite free from œdema. Upper lip considerably swollen, but no bleeding from the mouth nor appearance of biting the tongue. She had suffered from diarrhœa in the mornings for some days past. She was quite unconscious. Pupils dilated to to $\frac{3}{16}$ of an inch, fixed and immovable on the application of light. I passed the catheter and drew off some urine, which I found was densely loaded with albumen.

On examination per vaginam, the os was found dilated to about the size of a shilling. The cervix was soft and dilatable, though rather thick. Meanwhile slight pains came on at regular intervals of about fifteen minutes. I sent for chloroform, and also for the following mixture:—℞ Potassæ acetatis, gr. 240; aceti, fl. oz. $\frac{1}{2}$; aquæ ad fl. oz. vi. M. et S. Sig., A tablespoonful every four hours.

I waited till six o'clock, but as no convulsions came on, I left, giving orders to send for me in case of a recurrence of the fits, and directions that a horn-spoon should be placed between the teeth of the patient should a fit come on during my absence, and that she should be kept from injuring herself.

At 7.50, I was hurriedly called to see my patient, who had been seized with a fit of very much greater severity than any of the preceding. While in this fit she had bitten her tongue very badly, and since she came out of it, she had become wildly delirious. I arrived at 8.15, and found the patient tossing about in a very wild and restless manner. The pains had gone completely away since half-past six o'clock. I waited with her till 8.45, when she was seized with another fit. The spasms were tonic, general, and of great severity. The head was turned to neither side, nor was one half or limb of the body more affected than the other, except that both eyes were turned powerfully to the left. I held a large horn-spoon between the teeth, and yet the patient's tongue bled a little. The face became intensely congested during the attack, and the breathing deeply comatose. So intense was the coma and congestion, that, though at first I attempted to modify the fit by administering chloroform, and though I cannot charge myself with having any superstitious fears of the evil effects of the inhalation of chloroform, yet I dared not administer it during the seizure for fear of cerebral effusion. The attack passed off gradually in about ten minutes.

From 8.45 to 11 o'clock, only one pain, of no great intensity, and no fit whatever, occurred. About ten o'clock I happened to meet Sir James Y. Simpson, and mentioned the case to him. He advised, since the cerebral congestion was so intense, to bleed from the arm, and then try chloroform. I returned to my patient a few minutes before eleven, and immediately after my arrival, a fit, of still greater intensity than had as yet occurred, came on. The face was literally black, so deep was the congestion; and on recovery from this fit, an examination per vaginam revealed no appreciable change on the os, which was still little more than the size of a shilling-piece. I then at once proceeded, with the aid of my friend Mr Copland, to perform venesection, and drew fully twelve ounces from the right median cephalic vein. It was interesting to notice the change in colour which the face underwent during the extraction of blood. The flushing of the face passed almost entirely away. Scarcely, however, had I bound up the arm, when another convulsion came on; but this time it was not nearly so severe, nor was the congestion of the face nearly so deep as during the former attacks. Instead of being purple, it was only pale red. About five minutes after recovery from this fit, a pain supervened; and on examination per vaginam, during and after it, I was struck with the very remarkable and astonishing change in the dilatability and softness of the os and cervix uteri. On distending it a little with the fingers, I found it yielded so readily, that I felt convinced that I could safely introduce the hand at once, and turn. Mr Copland put the patient under the influence of chloroform, and, while he did so, I kept dilating the distensible os with the hand; and by the time the patient was completely over, I found I was easily able to pass the hand through the vagina and os into the uterus. The presentation was a vertex in the left occipito-anterior position.

I then ruptured the membranes, and with a little difficulty succeeded in seizing hold of and bringing down the right foot. In spite of every effort to the contrary, however, prolapse of the cord took place at the same time. I was therefore left no alternative but to hasten the labour as much as possible, in order to save the life of the child. During traction upon the body of the child, the uterus was pressed upon, and irritated as much as possible through the abdomen, with the view of keeping it contracted. Little difficulty was experienced till the head engaged in the brim of the pelvis, when, owing partly to a rather bulging sacral promontory, and partly to the anterior lip of the cervix getting jammed between the head and the symphysis pubis, the head became so tightly fixed (the face looked towards the left sacro-iliac synchondrosis), and I had to use so great an amount of traction in consequence, that, knowing the existence of prolapse, and feeling the cord had now ceased to pulsate, I was about to desist for a time from hurrying the labour, and to give the child up for lost, when a slight tremor of its body made me aware that it still lived. Another effort of traction pulled the head through the passages. The labour was ended about a quarter to twelve o'clock noon. The left parietal bone of the child was deeply indented with the traction of it over the sacral promontory. After the diligent application of the ordinary means of resuscitation, the child breathed well, and cried lustily. The placenta separated of itself very shortly after the delivery of the child. There was a little hæmorrhage, which quickly yielded to the usual means.

The pulse was now 120, the pupils still widely dilated and fixed. An hour after delivery, pulse 86; pupils still dilated and fixed; patient still unconscious; no recurrence of the fits.

At 5 P.M.—Patient sleeping rather heavily, no recurrence of fits; pupils still dilated, though now somewhat sensible to light. Had been rather unmanageable and outrageous at times, and it was only with considerable difficulty she had been got to take the medicine.

At 9 P.M.—Consciousness slowly returning; patient still quite blind; pupils getting much more sensible to light. She took from me quite pleasantly some egg-flip which I gave her. Found, on passing a catheter, about 10 or 15 ounces of water in the bladder, which on testing was found still intensely albuminous.

*17th*, at 1 P.M.—Patient very much better, but complains of feeling very tired and sore. She is unable to turn herself in bed or move her limbs. Sight now returning; pupils natural; water only slightly muddy on being heated and nitric acid added to it. Pulse 72, of fair strength.

8 P.M.—Patient quite conscious, and sees quite distinctly. A

large quantity of water removed by catheter. Pulse 72, of fair or rather good strength.

18*th*, 11 A.M.—Had passed an excellent night. Took in the morning a dose of castor-oil, which had operated well. Albumen considerably diminished, but still present.

For the next two other days, the patient was unable to move herself when she required to pass water or to get her bowels emptied, so that great care had to be taken to keep her clean and dry. But, on the whole, she made a good and rapid recovery. Examination of her urine to-day (24th November) revealed still a considerable amount of albumen, though, on careful examination, I failed to find any tube-casts.

*Remarks.*—I have presumed to lay this case before the Society chiefly on account of the remarkable effect which bleeding had upon the dilatation of the os uteri. Educated at a period when perhaps too little attention was paid to bloodletting, except to condemn it, I confess myself to have been previously a rather decided sceptic in regard to its efficacy for such a purpose. But from what I saw in this case, I feel bound to declare that, after all, we possess no other means which can for a moment cope with bloodletting in its effects upon the dilatation of the os.

It is a good example of the truth of the common assertion, that no belief, custom, or mode of treatment, ever took a general hold upon men's minds, except there was in it a very considerable amount of truth. I feel assured that bloodletting never would have had such a hold upon the profession, had its effects not been very beneficial indeed; and that, though our immediate predecessors in the medical art went too far in recommending the abstraction of blood, yet, from our extreme views in the other direction, it is a serious question whether we do not frequently deprive our patients of a very valuable therapeutic means. We are warned, it seems to me, by such cases as this, and many others of a like nature, to reconsider our position, and, instead of condemning bloodletting in a wholesale fashion, to select proper cases for it, and use it for these, and these only.

But besides acting beneficially as a means of relaxing the os, the bloodletting had a manifest effect in relieving the cerebral congestion, as evidenced by the change in colour which the face underwent. At the same time, it mitigated very markedly the severity of the convulsions; for the fit, which supervened immediately after the bloodletting, was not nearly so intense as the one immediately preceding it. Indeed, it was with the view of mitigating the cerebral congestion, and lessening the severity of the fits, that I opened the vein in the arm.

Another peculiarity in this case was the complete amaurosis. Amaurosis is a very frequent complication of puerperal convulsions, with albuminuria, as has been pointed out by Sir James Y. Simpson, in the first volume of his Obstetrical Works. But it is usually partial, and but seldom complete as in this case. The amaurosis is, however, almost always, as in the present case, temporary. A curious case, reported by Dr Ingleby, is referred to by Sir J. Y. Simpson, in a footnote, page 825, volume i. of his works, in which puerperal convulsions occurred during the first labour, but in the subsequent confinements their place seemed to be taken by complete amaurosis, which lasted the whole period of labour. The blindness, occurring as a complication of albuminuria, is not usually accompanied by any structural change in the eye or its appendages.

Headache is also a most common and most troublesome symptom, and is accompanied with more or less complete loss of consciousness. It is curious that, occasionally, before convulsions really come on, there may exist such a disordered condition of the intellectual powers, that the patient has no recollection whatever of what has taken place, although she speaks and acts quite naturally, and complains only of a severe headache. I recollect attending a case five years ago, when acting for Dr Bryce, which forcibly impressed this upon my mind. The convulsions came on after the labour had been completed for more than an hour, and there was no œdema or other sign to suggest the likelihood of albuminuria. She, however, complained very much of intense headache during the whole of the period of parturition, and had a good deal of that tendency to shiver, which we find so frequent during labour. But on being interrogated after recovery from the convulsions, she had no recollection whatever of anything that happened during the whole of her labour, though all the while she seemed to me to act and talk quite rationally.

As to the cause of those terrible convulsions accompanied with albuminuria, on looking into the authorities, the state of our knowledge seems eminently unsatisfactory. The theory of urea in the blood seems without any good foundation; the same also must be said of Frerich's theory of carbonate of ammonia resulting by fermentative change from the urea of the blood. That it should be due to a poison existing in the blood in all cases, seems to be strongly negatived by such as yield readily to bloodletting, as the one we have recorded. For, if a poison existed in the blood to such an amount as to cause convulsions of such terrible intensity, it is unreasonable to suppose that the fractional part of it eliminated in ten or twelve ounces of blood could have any beneficial effect on the general result. Neither will the theory of vascular pressure or tension alone suffice as a cause, for we so frequently see the very worst cases of so-called uræmic convulsions occurring, as in this case, without dropsy to any extent. I have myself treated other two cases of very severe uræmic convulsions, and in neither of them was the amount of dropsy other than exceedingly trivial. The pressure in some way or another must have, nevertheless, some relation to the convulsions, else how could bloodletting be so markedly beneficial as it is often found to be? It may be, as has been suggested, that the interference with the proper elimination of nerve-tissue waste so interferes with the nervous system as to render it specially supersensitive for the time being, and that a small amount of pressure added to this condition is capable of producing those terrible disturbances in the nervous system, under the action of comparatively slight exciting causes, such as the contraction of the gravid uterus. Be this as it may, however, the irritation of the contraction of the gravid uterus seems to increase the tendency to the fits very much, and to warrant us in endeavouring to hasten the emptying of the uterus by all legitimate means.

*Dr Pattison* said that in his younger days bleeding was much practised in cases of rigid os, and he had seen the benefit of bloodletting in cases of convulsions.

*Dr Keiller* said that one of the first cases he was called on to attend was one of convulsions. He bled freely, and was satisfied that the patient's life was saved in consequence, and he believed that nowadays we did not bleed often enough.

*Dr Murray* mentioned a case in which, after puerperal convulsions, one eye was much damaged, and sight was never recovered. The patient had albuminuria.

*Mr Pridie* had met with some seven or eight cases of puerperal convulsions, which all recovered after bloodletting. In one of the cases there was complete amaurosis.

*Dr David Gordon* said that bleeding was of benefit in other cases of convulsions. He remembered a case of uræmic convulsion in a young man, who never had another after being bled.

*Dr Thomson* said he had seen a great many cases of convulsions, and he believed that in many cases the hæmorrhage which followed delivery was an important element in the treatment. In such cases he was in the habit of giving the bitartrate of potass in teaspoonful doses every hour.

*Dr Bruce* said he had not met with any case of puerperal convulsions for some time. He relied very much upon the good effects of bleeding, and his cases had all done well.

*Dr Cairns* thought the profession was pretty much at one in regard to the propriety of bleeding in such cases, and its beneficial influence might be explained in this way, that the system could bear a certain amount of poisoning, but whenever a certain point was passed convulsions took place, hence the relief afforded by bleeding even in small quantity. In rigidity of the os he did not think bloodletting of much consequence after all.

*Dr Bell* said that bleeding had a marvellous effect in dilating the os. It used to be common to bleed to syncope, and he had never seen any evil effects from it.

*Dr Duncan* said he agreed with the general tenor of the paper. He remembered when there was nothing else but bleeding in cases of convulsion; then chloroform took its place. But there was another method which he had seen used with success, and that was simply doing nothing at all. He had seen a large number of cases, and was of opinion that in one set of cases, where there was much lividity of the face and unconsciousness, bleeding was advisable, and chloroform was the reverse of useful; but in another class of cases, where the fits were violent and frequent, chloroform was beneficial. In other cases, again, doing nothing was useful, especially where labour is going on quickly.

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# Part Fourth.

## PERISCOPE.

**On the Cause of Intermittent and Remittent Fevers.**—In a very long and interesting paper, which, in these days of rampant germ-theories, will be read with great interest, Dr Salisbury of Cleveland, Ohio, develops his theory of the cause of intermittent and remittent fevers. Conscious, he says, of the purely hypothetical character of all the theories hitherto formed in regard to the origin of these diseases, he was induced, by a long experience of camp diseases, of various forms of blight in plants, and of many inquiries into the causes of putrefaction, fermentation, gangrene, etc., as well as by a considerable acquaintance with the matters floating in the atmosphere, to investigate practically the cause of fevers assuming an intermittent type. In 1862, intermittent fever appeared during the month of May in the rich but marshy valleys of the Ohio and Mississippi, but assumed no great extension prior to July and August of the same year. Up to the 1st of July the weather was extremely wet, but during July, August, and September, there was no rain at all, and an extreme aridity everywhere prevailed. Coincident with this aridity, intermittent fever increased and extended so rapidly as to leave scarcely one family unaffected throughout these marshy districts. Dr S. commenced his investigation by examining the salivary and mucous secretions expectorated in the morning by those living in these districts plunged in a malarious atmosphere. In these he found a quantity of zoosporoid cells, of animalcules, diatomes, desmidiæ, cells and filaments of algæ, and spores of fungi. But the only corpuscles uniformly found, and generally in great abundance, were small oblong cellules, isolated or agglomerated, which had a distinct nucleus inclosed in a smooth cellular envelope, a clear space intervening between them. These characteristics at once convinced him that they were the cells of a species of alga, chiefly resembling the *palmellæ*. While the other corpuscles were found at all and every elevation, these palmelloid cells were only found in malarious districts where fever prevailed. Dr S. was then led to look for their source and characteristics. On suspending pieces of glass above marshy pools at night, removing [illegible] before sunrise next morning, their under surfaces were found covered with drops of water containing many cells similar to those found in the expectoration, but none of the above-mentioned oblong cellules, which, on the other hand, were always found in considerable number on the superior surface of the glass. On passing in the course of these inquiries over a rich prairie ground, recently dried and broken by the footprints of cattle, Dr S. was sensible of a peculiar dry feverish sensation in the gullet, which sometimes extended to the pulmonary mucous membrane, and he discovered that on these occasions his expectoration uniformly contained numbers of the oblong cells already described. Thus attracted to those recently dried spots trampled by cattle, Dr S. discovered on the recently exposed surfaces of the cracks a sort of grayish mould, and suspending his glasses over these spots he was delighted to find next morning that their under surfaces were now covered with numbers of the long-sought-for cells. Samples of the turf and fresh mould from these spots, when examined microscopically, showed that the incrustations with which they were covered consisted of aggregations of the cellules so constantly found in the expectoration of those living in marshy regions, and these were subsequently found to be algoid cells of the palmelloid type producing several varieties of mucedinous fungi. Various experiments proved, 1st, that cryptogamic spores chiefly rose into the air during the night, and fell shortly after sunrise; 2d, that in the various districts examined 35 to 100 feet was the height to which these spores were carried; 3d, that above the height to which these nocturnal exhalations attained there was no intermittent fever; 4th, that during the day the air of miasmatic districts contained none of these palmelloid cells. Various experiments made by Dr S. and his friends proved that a continuance of fifteen minutes or longer in these marshy spots was always followed by the almost immediate development of dryness and febrile heat in the throat, with a tendency to hawk and spit, which lasted for some time (two hours in one case) after leaving them; and these symptoms were constantly accompanied by the presence of the above-mentioned palmelloid cells in the expectoration. Dr S. relates various cases showing that intermittent fever does not attack those who live at a certain elevation above the marshy ground, nor who live beside marshy places, unless the wind blow towards them over these spots; and he connects these facts with the occurrence of palmelloid vegetations by showing that, where the ground covered by this vegetation was strewn by his orders with quicklime or with straw to the depth of several inches, no palmelloid cells were detectable on glasses suspended above the soil, and fever ceased to be developed. Dr S. also states that, where the soil is not calcareous and the water is soft, the palmellæ are white, or pale-yellow, or green, and the fevers are less congestive, milder, and more amenable to treatment than they are where the soil is calcareous, the water hard, and the palmellæ of a deeper green or brick-red colour; in these latter districts the fevers are more congestive in character and less amenable to such remedies as quinine, arsenic, or iron, requiring them to be conjoined with diuretics, diaphoretics, expectorants, and alterants for their successful treatment. These latter eliminant remedies Dr S. is, from his peculiar theory, led to regard as of most importance as curatives of intermittent fever, while quinine, etc., he only regards as tonics of the ganglionic and other systems, fitted by their actions to prevent the development of subsidiary symptoms (the actual fever), while the sporular poison is being eliminated. His investigations lead him to believe that intermittent fever is caused by the development of these palmelloid spores in the body, the proof of which is their detection in the urine. And the most singular fact in connexion with this theory is the development of the symptoms of fever in the persons of those who were exposed to the inhalation of these palmelloid cells, far removed from their original source, and in districts not regarded as malarious. Of this Dr S. gives several instances. The theory is ingenious; it adapts itself to present prevailing views, and is supported by many remarkable statements, which, for want of further proof, we forbear to designate as facts. Gathered in America, these statements, if true, must be susceptible of proof elsewhere. Britain has many malarious soils, and many careful investigators, and her medical talent could not be better employed than by settling the nature and mode of origin of a disease which is the cause of so many deaths and of so much expense in attempts at cure. The paper from which this is quoted will be found *in extenso* in the *Revue des Cours Scientifiques de la France et de l'Etranger*, No. 49, 6 Nov. 1869.

ON DIGITALIS IN HEART DISEASE.—Dr Sidney Ringer considers that digitalis is eminently useful in cases of enlargement of the heart accompanied by dilatation, with dropsy, general venous congestion, and a frequent, feeble, fluttering, and irregular pulse, with or without valvular disease, and quite irrespective of the nature of that valvular disease. That is, he thinks it just as useful in obstructive or regurgitant aortic disease as in mitral, provided the above symptoms, and especially the irregularity of the pulse, be present. He thinks it of importance to give no more of the drug than is necessary for the relief of the patient; and that, in many cases, one drachm of the infusion twice or thrice a day will be sufficient. Should, however, no improvement be observed after the lapse of four or five days, the dose must be increased. In other cases, both in young and old patients, an earlier stage of this severe disease may be present, characterized by palpitation, dyspnœa, tumultuous and often forcible action of the heart, with feeble and frequent but generally regular pulse, no dropsy, and no lividity of the skin. A mitral murmur, often accompanied by apex thrill, is generally to be detected, and sometimes the signs of aortic valvular disease are also present, but these signs do not contra-indicate the use of digitalis in such cases, in which it is most useful in controlling the heart's action, increasing the firmness and fulness of the pulse, and relieving the dyspnœa. Where no dropsy is present, there seems to be no diuretic action produced. Dr Ringer believes that in all such cases digitalis acts by restoring order to the heart's movements, thus removing any regurgitation caused by irregular action of the columnæ carniæ, with the venous congestion and dropsy thereon depending. Hence it can only remove, with completeness, symptoms depending on dynamic regurgitation, leaving untouched those arising from organic valvular disease; so that, in a complicated case, it can only be productive of partial benefit, being only useful in proportion to the degree in which the dropsy, etc., is due to the irregular action of the heart, and is independent of organic disease. Digitalis he considers very useful in cases where persisting palpitation, with or without slight mitral murmur, have been much aggravated by an attack of bronchitis, but of no use whatever where pre-existing lung disease (emphysema) has become complicated by secondary heart affection and dropsy. He also regards it as of no use, and possibly hurtful, in cases of aortic disease (obstructive or regurgitant), accompanied by palpitations, dyspnœa, and dropsy, but where the heart and pulse both beat regularly and without any intermission, the latter having frequently the characters significant of aortic regurgitant disease, with a pale waxy skin, no lividity, and no fulness of, nor regurgitation into, the jugular veins.—*Practitioner*, Jan. 1870.

FURROWS OF THE NAILS AFTER SICKNESS.—Dr Wilks and others have been recently directing attention to the fact, said to have been first pointed out by Dr Beau of Paris, that, after severe illness, there are frequently deep notches running across the nails. This is a fact well known to every horsedealer in Britain, who all take special care to have the hoofs (nails) of those horses that have been recently severely ill, particularly neatly shod, and well rasped, to obliterate the tell-tale mark. It depends, for origin, upon the same physiological cause which enables us to reckon the ages of trees and of cows. We know that, during the cold weather of winter, the growth of trees in our climate is arrested, hence a transverse section of their stems exhibits a succession of rings of annual growth, by which the number of summers they have lived may be calculated. In like manner, the cutaneous circulation of all animals seems to be more or less impaired by the cold; and in animals with permanent and smooth horns, such as the cow, this is shown by a thinner layer of horn being formed during winter than during summer, so that their age may be, and is calculated, by counting the rings at the base of each horn, three years being added for the tip, which being more solid (having less core) is less affected by this cause, and wants those rings. In all animals with hoofs these are always found notched in severe illness; the horns, we believe, are seldom so much so as to introduce any disturbing element in calculating the age; but observations on this point are wanting. Smoothed and polished horns in cattle, and well-rasped hoofs in horses, are both suspicious—the former are probably aged, the latter have probably been recently dangerously ill. In man himself the ordinary finger and toe nails are usually so thin as not readily to be distinctly notched by a trifling interruption to their growth. But in the thicker thumb-nails, and on those of the great toe, such marks are very frequently observed after severe illness, and they take nearly six months (on our own nails quite so much) to grow off and disappear at the free edge. The same cause (partial cessation of growth from peripheral vascular debility) causes the hair to fall off after fever, or any serious illness, or during the course of phthisis and other fatal chronic diseases. In the case of the latter the growth wholly or partially ceases, the hair gets torn out by brushing, etc., and is not reproduced, or only partially succeeded by a scanty, dry, imperfect growth. In the former case, if the hair be kept long, its own weight, or the act of brushing, tears it out, and temporary baldness is the result; but if the hair be kept short and much brushing avoided, there being no actual cessation of growth, but only a temporary notching of the hair, it does not break off till the new growth of returning health is already some lines above the skin; and much unnecessary anxiety is thus prevented, and many shillings spared, which would otherwise have been thrown away on useless washes, and which are certainly, in such cases, better spent on food.

HISTORY OF THE ITCH.—Captain Dabry assures us that the disease was known by the Chinese four thousand years ago, and was called *tchong-kiai*, signifying pustules formed by a worm.

Avenzoar, in the twelfth century, is reputed to be the first to have reported the presence of a little animal in the disease, so small that it could hardly be seen, covered by the epidermis, and escaping as soon as an opening was made. The parasite was afterward recorded by many authors, and figures given representing it.

In 1812, Galès published the first monograph upon the subject, described the animal, and gave a plate representing it. This plate was reproduced in books for fifteen years, as an exact representation of the parasite, but it did not correspond with the figures given before, and further investigation threw doubt upon its accuracy. Alibert and Biett tried in vain to find it, and finally Raspail discovered that the figure was an exact representation of the cheese-mite. Incredulity again returned to the profession, and, as lately as the year 1821, Lugol offered three hundred francs to any one who could show the animalcule causing the itch. This was finally done, first, in 1834, by François Renucci, assistant of Alibert, who extracted the insect in the presence of numerous spectators, and thus put an end to the controversy.

Since that time, the animalcule has been pretty generally considered to be the sole cause of the disease, but even now some authors of note deny to it the *rôle* of causality. Divergie, for example, in his "Traité Pratique des Maladies de la Peau," Paris, 1863, arrives at these two conclusions:—

1. That the itch may be a spontaneous malady.
2. That if the acarus is constantly present in the itch as a coincidence, yet the theory of its existence can just as well admit that the insect is a morbid product as that it is a morbid cause and agent of transmission.

VEGETABLE PARASITES.—The vegetable parasites causing cutaneous eruptions have not shared so hard a fate. Their history includes but little more than thirty years, and the labours of Gruby and others in establishing them, and of Bazin in popularizing the knowledge concerning them and instituting a rational treatment, have been most successful.

But still conviction is not universal, and no less a name than Erasmus Wilson heads the van of dissenters, denying the parasites as a cause of disease or of contagion (positive experiments in proot to the contrary notwithstanding), and looking upon them only as secondary formations—as a so-called "phytiform degeneration" of the tissues and secretions of the body.

The labours in parasitology have, however, been successful, and their results mark a bright era of progress for dermatology.

As to the discovery of new parasites causing skin diseases, a little has been done, but the discoveries have been, as a rule, of slight practical importance. Perhaps the most positive advance in this direction (to go pretty far back) was, the establishment of the identity of the parasite (the Trichophyton of Malmsten) in the three diseases, sycosis, herpes circinatus, and herpes tonsurans, and a recognition of the fact that the same parasite may give rise to seemingly different disorders according to the nature of the soil upon which it develops. Later (1864) and still more important is Koebner's discovery that this same parasite is the cause of the disease described by Hebra as eczema marginatum. Pick has recently studied this disease exhaustively, and made it the subject of a long and interesting article in the first number of his journal.[1] Hebra responded, upon which Pick closed the discussion and proved his point, placing eczema marginatum among the parasitic diseases, and proving it to be a herpes tonsurans vesiculosus joined with an intertrigo, thus forming a compound disease.[2]

ELEPHANTIASIS ARABUM.—Among the more positive practical advances of recent date may be mentioned the fact, now so well established, of the marked influence exerted upon a limb affected by elephantiasis arabum brought about by arresting the circulation through the main artery of the part. Many successful cases have been reported during the last few years. Ligature of the artery has been usually resorted to, but that this is not always necessary, is proved by Vanzetti's case,[1] where a cure was effected by digital compression alone.

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[1] Archiv für Derm. und Syphilis, 1869, 1.

[2] Ibid., 3.

The last summary on the subject is given by Dr Fisher,[2] of Hanover, who tabulates the cases which had occurred up to date, twenty-one in number. Out of these, eleven are reported cured, nine entirely, and two very nearly. In two cases there was improvement. In seven no improvement or death was the result; and one, Vanzetti's case, was cured by digital compression. The table proves that this method, although not necessarily successful, is yet an improvement on the treatment previously in use. The idea of curing elephantiasis by interrupting the circulation of a limb originated, it appears, with Dr Dufour, since, in an article written by him in the *Gazette Hebd.*, 1863, he states that his first attempts in this direction date back thirty years; in other words, he was already experimenting the idea in 1833.—*Report on Dermatology, in New York Medical Journal*, Jan. 1870.

RED BLOOD-CORPUSCLES OF OVIPAROUS VERTEBRATA.—Mr W. S. Savory, in a paper read before the Royal Society, states that, of all vertebrata, the red corpuscle is, in its natural state, structureless. When living, no distinction of parts can be recognised; and the existence of a nucleus in the red corpuscles of ovipara is due to changes after death, or removal from the vessels. As the oviparous red cell is commonly seen, the existence of a "nucleus" is too striking an object to escape any eye, but that it is so is due to the circumstances under which the corpuscle is seen, and the mode in which it is prepared for examination. It is possible to place blood-cells under the microscope for examination so quickly, and with such slight disturbances, that they may be satisfactorily examined before the nuclei have begun to form. They may then be shown to be absolutely structureless throughout; and, moreover, as the examination is continued, the gradual formation of the nuclei can be traced. The "nucleus" first appears as an indistinct shadowy substance, usually, but not always, about the centre of the cell. The outline of it can hardly, for some seconds, be defined; but it gradually grows more distinct. Sometimes it never forms so as to be clearly traced in outline, but remains as an irregular shapeless mass, in its greater portion very obscure. Sometimes only a small part, if any, of an edge can be recognised, most of it appearing to blend indefinitely with the rest of the cell-substance. Sometimes it happens that in many corpuscles the formation of a nucleus does not proceed even so far as this. No distinct separation of substance can anywhere be seen, but shadows, more or less deep, here and there indicate that there is greater aggregation of matter at some parts than at others. Occasionally some of the cells present throughout a granular aspect. When the nucleus is well defined, the cell-wall is strongly marked; when one is confused the other is usually fainter. This, however, does not apply to colour; on the contrary, when the nucleus is least coloured it contrasts most strongly with the surrounding cell. As a rule the wall of the cell is more strongly marked than the nucleus.

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[1] Gazette des Hôpitaux, 1867.

[2] Virch. Archiv, 46, 3, p. 328.

To the objection that the nuclei are present all the while, but are at first concealed by the surrounding substance, the author replies that, when one cell overlaps another, the lower one is seen through the upper clearly enough to show that the substance of these cells is sufficiently transparent to allow of a nucleus being discerned if it exists. When a nucleus is fully formed it hides that portion of the outline of a cell which lies beneath it.—*Annals and Magazine of Natural History*, July 1869, p. 69.

RED AND WHITE CORPUSCLES IN BLOOD-STAINS.—Dr Joseph G. Richardson, of Philadelphia, has attended closely to this matter for the purposes of medical jurisprudence. If the stain, at any time in question, be of blood, is it of man, or of one of the lower animals? For moistening the dried blood-clots the author has made use of pure water. He refers to some previous investigations he has made ("Pennsylvania Hospital Reports," 1869), saying, "In the progress of some researches upon the distention of the white blood-cells, when acted on by water, I have often incidentally noticed that many of the red corpuscles become, after a time, so transparent and colourless by the solution and abstraction of their 'hæmato-crystallin' that they are quite invisible under a power of 400 diameters, and appear to be in reality dissolved, as stated by Prof. Wyman, M. Ch. Robin, and other authorities; yet, when closely scrutinized under a 1-25th immersion objective, their faint transparent outlines can still be detected, thus confirming Prof. Beale's assertion that, 'with the higher powers, not only do we meet with extremely minute corpuscles, but many of them are so very transparent that they could not be seen at all under a low power. Extremely transparent bodies are demonstrated under high powers, which would certainly be passed over by those in ordinary use.'" The author goes on to say that his "results seem to prove a very marked difference in density, if not in constitution, between the external and internal portions of the blood-disks." Yet he would not go so far as to affirm of the red corpuscle that it has a proper cell-wall.

Besides human blood, that "of an ox, sheep, pig, chicken, turkey, and canary bird, most of them dried in a thin film upon a slide, and all dried in a mass upon paper or muslin, were carefully examined, and little difficulty found in distinctly perceiving that the colourless stroma, with its 'straight or slightly waving filament, sometimes more fibrous, sometimes more wrinkled and homogeneous' (Virchow), so long mistaken under lower power for a mass of fibrin, was actually an aggregation of decolourized red corpuscles, with rare filaments of fibrin, and white blood-cells embedded in it." Dr Richardson concludes that, "as during the last three or four years opticians have furnished immersion lenses of 1-25th and 1-50th of inch focal length, which, with the highest eye-piece, give an amplification of about 2500 and 5000 diameters respectively, thus rendering, with the former, the apparent size of a red disk from fresh human blood five-sevenths of an inch, while that of a corpuscle from ox blood is but half an inch across, and consequently little more than half the area, as seen upon the stage, it seems as if any careful observer might now, with the aid of such objections, be qualified to pronounce a positive opinion."

The author, by his numerous special investigations, disproves the possibility of any considerable variation in diameter of the corpuscles, of the same blood, occurring by more or less rapidity of desiccation—a plausible objection of Virchow's. He affirms, by calculation given, that "the outer portion of the corpuscles" has no elasticity. Although the red disk, rendered spherical by the action of pure water, is thus altered, he determines that "its parietes undergo no real dilatation in the process; further, the corrugated appearance assumed by the corpuscle when any portion of its internal constituent is removed by exosmosis affords some evidence that, however much the cavity is decreased, its limiting membrane suffers no actual diminution in superficial area."

"That the blood-corpuscles of a few mammals approach so nearly in size to those of man as to render their distinction doubtful," is of little importance, for justice rarely requires but that a positive diagnosis shall be made between human blood and that of animals which are commonly slaughtered for food, of all of which "a first-rate 1-25th inch objective will enable us to determine easily and beyond all question."—*American Journal of the Medical Sciences*, July 1869, pp. 50-58.

# Part Fifth.

## MEDICAL NEWS.

THE REVIEWER of Mr Child's *Essays on Physiological Subjects* writes to us as follows :—" Mr Child complains that in our notice, in the January Number of this Journal, of his 'Essays,' we cursorily alluded to the materialism evinced in his 'Physiological Psychology.' He declares he is not a materialist. This is sufficient to set the question at rest. Mr Child does not impute any hostility to the reviewer, which, from the favourable nature of the *critique*, is only just; but he points out several passages in his work of an anti-materialistic tendency—pp. 156, 177, 273. In one of these, there is a repudiation of atheism, which, however, was thought quite consistent with materialism by many of the deists of the last century. The allusion in our review was founded on the preference which we believed Mr Child gave to the materialistic view at pp. 289–90. If no one is a materialist unless he holds that his favoured hypothesis can be established beyond the reach of doubt and cavil, then we conceive that there are no materialists at all. Nevertheless, we are sorry if this imputation has caused any uneasiness to Mr Child. We would be the last to invoke in any way the intolerance of the public to anybody's speculative views, however removed from our view."

MODE OF IMPROVING THE HEALTH OF TOWNS.—At a recent meeting of the Glasgow Police Board, convened specially for the purpose of discussing the sanitary condition of the city, the following admirable speech, much of which is applicable to all cities, was made by Mr URE, who said—"I beg to move that the Board of Police adopt the recommendations contained in the first division of the report of the deputation to Bristol and other English towns, and that, so far as practicable, effect be given to them with a view to improving the sanitary condition of the city. The second division, relating to the disposal of the sewage, I propose to leave until that much-debated question comes to be discussed. The report itself is mainly descriptive, and if there be any point requiring explanation, some of the other members of the deputation or myself will be happy to answer any questions that may be put. But in the meantime I believe I will best consult your convenience if I pass at once to the consideration of the recommendations which we have thought proper to make, and I will do so in the order in which they occur. The first recommendation has reference to what is now too common in Glasgow, and I am sorry to say as frequently found in tenements of comparatively recent construction as in those of a much older date, viz., close and ill-ventilated lobbies and staircases, the common stair being in many instances placed in the centre of the building, and from it, on every landing, a lobby branching off. In other cases you have the stair at the back of the tenement, and off every landing a lobby, at the further end of which you will sometimes find a second lobby spreading out at right angles to the first, forming that construction which will be best understood as resembling the letter T. The first lobby is very imperfectly lighted from the staircase. The second is dark even at noonday. Yet these are the passages to six or eight, or even as many as ten, dwellings on each flat. The atmosphere gets foully tainted, and enters the dwellings by every opening door. If infectious disease breaks out, it is certain to spread through the whole land. Isolation, in such cases, is impossible—short of displacing two or three hundred of the population. That is the difficulty you have to meet. Our recommendation is, that the staircase should have an opening to the roof of sufficient size, and that through-and-through ventilation be secured in the lobbies. You will all admit the desirability of this; its practicability as regards the staircase is quite apparent; with the lobbies there may be more difficulty and more expense; but that it can be done is evidenced by what Dr Gairdner and Mr Carrick have effected in as bad an instance as we are likely to find. I refer to a four-storey tenement in Middleton Place, off Garngad Road, where they have compelled the introduction of a ventilating shaft up through the inner dark lobbies of one of these objectionable tenements. I had the testimony on the spot of one who has lived in it for the last five years, that since this was done the air is not nearly so stifling as it used to be. In this instance there is only a shaft at one end of the transverse lobby. Our recommendation would instruct that there should be one at the other end also. Now, the question arises, have we legal power to enforce this recommendation? I think we have in the Public Health Act, for in it I find that 'any defect of ventilation rendering any inhabited house, building, or premises, or part thereof, injurious to the health of the inmates,' is defined 'a nuisance,' and a certificate to that effect from the Medical Officer enables the local authorities to bring the case at once before the Sheriff or any magistrate, who may grant a decree to provide sufficient ventilation; and if the nuisance proved to exist be such as to render a house or building unfit for human habitation, he may prohibit the using thereof for that purpose until it is rendered habitable, or to do otherwise as the case in his judgment may require. Our second recommendation has reference to the dwellings in which the poorer classes generally reside. We say that they are too small for the number of occupants, and the people being mostly ignorant of the necessity of change of air, that some simple means of ventilation beyond their control be provided, and that the sanitary officers be instructed to see that it is kept in working order. The same legal powers that would enable us to effect the improvement in the staircase and lobbies would cover this also; and I am sure I have not to say a single word as to the necessity. Whoever has visited these crowded houses, even during the day, will be sensible of the foul air they usually contain; but whoever has visited them in the middle of the night, as I have done, will bear me witness that the atmosphere that meets you as the door is opened is like the breath of pestilence. Where the apartments are too small, we have power by our Police Act (clause 376) to compel single apartments to be connected, and made houses of two or more apartments; and where that cannot be done, to disallow their use as habitations altogether. Our third recommendation is in regard to privy and other accommodation; and this, although at first sight a very simple, as it is a very necessary matter, is nevertheless, probably, the most difficult to deal with of all. It is a trite saying, that you cannot have too much of a good thing, and I presume the opposite also holds, that you cannot have too little of a bad thing. Perhaps we have taken this latter view too much; I think we have; but the very best of these out-door conveniences, in the midst of the crowded population in our city, have been bad things, with one or two exceptions, and I would gladly welcome any change that would permit their use, without creating nuisance. We recommend that some deodorizing material be used in them once or twice a day, and, whatever the cost of this may be, I would strongly urge its necessity. In carrying out this recommendation, there will be places found where to get sufficient room to erect the convenience. Buildings will have to be taken down, but we must not stop for that. The cupidity of some has been such as to cause them to build on every inch of ground which rent could be taken out of, without regard to their tenants' requirements. There are closes, too, that I could point out, the wretched property in which is owned by from twenty to thirty different persons. Whatever change you wish to effect there, all these parties have to be taken into court. Some of them may live at a great distance from Glasgow; some may not be found at all—may be too poor to pay their share of the outlay. That was our experience before, when, seven years ago, we caused a large number of conveniences to be erected in the poorer districts. I had to ask this Board (amidst much opposition) to supplement the payment of them to the extent of £336, of which sum only £47 has yet been recovered from the parties liable. These are some of the difficulties that were in our way. It is not want of legal power to compel their erection: that is ample enough, both by the Public Health Act and the Police Act, and I believe even at common law. We indicate the extent of the provision we think needful (one seat for every five dwellings). This may seem too little, but it must be borne in mind, that the adult male population are mostly absent during the day, and so get accommodation elsewhere; and that the female portion, although provision has been made for them, have never taken advantage of it. I would not despair, however, in that direction; I think we have not given *them* sufficient privacy; and so, in any new movement, I should strongly advise that *that* should be considered. I would also suggest that we might, with great public advantage, exercise the power we have in the 353d clause of the Police Act, which authorizes the Board to erect water-closets or privies in such situations as shall cause the least inconvenience, and to defray the expense thereof, and of keeping them in good order. He also recommended that there be a supply of water upon every stair landing;—I am sorry to say that the water-supply in many of the poorer localities, although far better than it was some years ago, is not yet so full as it should be. But the Water Commissioners have generally hearkened to our remonstrance, and I have no doubt will do so still. Our fourth recommendation is, that the surfaces of all our courts and closes be laid with pavement, or other impenetrable or durable material. You would notice from the report how much importance the authorities of Bristol attached to this. And with their experience of its advantages, we were not surprised to learn that when they could not get proprietors to do it promptly, it was done by the Board of Health, and recovery of its cost effected by after proceedings, some of which are not terminated yet. By the 318th clause of the Police Act, 'the Master of Works may require any proprietor of a land or heritage adjoining to, and having a right of access by, any private street or court, to alter, repair, or renew foot-pavements therein to his entire satisfaction.' I therefore hope that not a moment will be lost in putting this power in operation. How can we expect cleanliness within the dwelling when every entrant is liable to carry with him the dirt from the court or close? And how can we expect the Cleansing Department to perform their duty efficiently in these unpaved and dirty closes? As in the other case I have referred to, so in this, it may be needful for the Board to supplement the cost. But with a climate so moist as ours, and with a population requiring so much a cleanly example set them, the Board must not hesitate to press on this needful reform. Our fifth recommendation is, that the Chief Constable take measures to compel the inhabitants to keep the common stairs and closes clean, it being found that filth is often thrown upon their surface that ought to be carried to the ashpits. My experience, when a magistrate sitting in the Police Courts, made me sensible how onerous this duty is. The policeman cannot always be on the watch; in his absence the offence may be committed; and, not knowing the offender, every tenant has to be summoned, and generally all deny a knowledge of the offence. This matter I leave with Captain Smart, in the full confidence that if it be possible to do more than has been already done, the duty will not be neglected. Our sixth recommendation was, that Dr Gairdner be requested to inform the Board whether, with his present sanitary staff, he could properly overtake a more extensive system of visitation to inculcate cleanliness and ventilation, and for the prevention of overcrowding. What other answer could be looked for from Dr Gairdner than that, with his present limited staff of inspectors, it would be impossible properly to overtake the duty? That answer I quite anticipated when I penned the recommendation. If the Sanitary Benevolent Association, to which your Lordship and myself were sent as representatives by the Town Council, thought it needful to have above a thousand visitors to carry on its operations, then surely eight men are far too few to perform efficiently the public service. Allow me, Gentlemen, a passing word on this now dissolved association—for I am grieved at the failure of what promised to be so powerful an auxiliary to us, and withal so noble an enterprise. I took a humble part with a few devoted men in trying to enlist our citizens, and to rouse the philanthropy of the churches, to this truly Christian work. But they seemed to think that to reach the souls of men, it was of no account in what condition their bodies were. A few notable exceptions there certainly were, but these exceptions were so few when set against the general apathy, as made it impossible for us to proceed. As a Christian community, we have yet to learn how near cleanliness is to godliness. We seem to see no difficulty in the alliance of physical uncleanness with spiritual purity. From all this I take leave to dissent. A dirty Christian appears to me a contradiction in terms; and my difficulty increases, if to find him I have to ascend a dirty stair to a dirty house, and get him breathing a foul dirty atmosphere. I say it with deliberation, that if the Church is to have all the success she desires in evangelizing the masses, she must never forget that the same God who made the soul also made the body. It is a lower work, no doubt, but it is His work, and as such He careth for it. The Scripture tract distributor should distribute sanitary tracts likewise. The Scripture reader should also read lessons of household and personal cleanliness. The city missionary should be able to tell of the advantages of pure water and pure air, as well as of pure religion and a holy life. To those who doubt the propriety of such a combination I recommend the Christmas number of the *British Workman*, in which they will find an article entitled 'My First Ministerial Difficulty,' and a picture illustrative of a minister of our pure and holy religion in his shirt-sleeves giving a workman's family a first lesson in sanitary reform. The next point touched on in the report has reference to the practice pursued so successfully in Bristol in stamping out typhus fever, and which we heartily recommend to the best consideration of Dr Gairdner and his staff of medical officers. We point out very emphatically the difficulties in adaping that practice to the crowded and unwholesome tenements in Glasgow. To my mind these difficulties are almost insuperable. When I explained to Dr Davies (which I fully did) our position, he frankly stated, in the presence of the deputation, that in our present circumstances he could do little for us, and therefore, whilst I urge with all earnestness that what we can do we do with all our might, I still feel it to be my duty to caution the public and the Board from entertaining unreasonable expectations. The last recommendation that we make may be looked upon as quite gratuitous, as the Board of Police have no power to compel either private builders or the Improvement Committee to provide the most desirable dwellings for the poorer classes; and therefore I will not take up your time in enforcing this recommendation. The question for us now to consider is, how can we improve the sanitary state of Glasgow, and by what means can it be effected? We have put before you eight specific recommendations. One of these, the last and probably the most important, must take the form of moral influence, by assisting to create on the subject of house accommodation a wholesome public opinion. Respecting the other seven, I have no hesitation in saying, that if we have the will, we will find the way to carry them out. But, Gentlemen, unless you agree to grant a considerable extension in two of our departments, I have no hope, none whatever, of doing any material good. One of these departments is that of Nuisances, which still requires what I asked for it ten years ago—a responsible head inspector, an assistant inspector, and a sufficient staff. The other department requiring extension, and it may be revision, is that of the Master of Works. Up to a certain point we come at present, both from the Police and Sanitary Departments, in reporting nuisances, and there we stop—the cases accumulate; the work is not done. The reporting officers come to think that, from some cause unknown to them, it cannot be done. They are discouraged and disheartened; we know the rest. Re-arrange the departments as you will, if you leave this one alone your work will be in vain. Twelve years now I have sat as Chairman of this Sanitary Committee. In all my life it is the only appointment I ever asked. I value it more highly than any office in your Lordship's gift, but if these reforms are denied me to-day I must cease to hold it; my portfolio must be taken up by another. I have attended with your Lordship to-day a meeting of the committee on our municipal extension, but this matter we have now on hand I deem of far greater importance. 'In the multitude of the people,' says Solomon, 'is the king's honour.' Most true; but not in a sickly, enfeebled, incapable multitude. We have dropped the city's old motto of 'Let Glasgow flourish by the preaching of the Word.' I offer you a new one—'Let Glasgow flourish by the practice of it.' Practice, in this case, is far better than precept—as much better as plenty is better than poverty, as health is better than sickness, as life is better than death. (Applause.)" Various other speakers took part in the discussion of the subject; and, on the motion of the LORD PROVOST, a committee was appointed for the purpose of more effectively improving the sanitary condition of the city, by carefully investigating the whole question, spreading information among the people, and advising with the authorities as to what ought to be done in special cases.—*North British Daily Mail*, January 13, 1870.

ROYAL HOSPITAL FOR SICK CHILDREN.—Dr Arthur Gamgee has been appointed an Ordinary Physician in room of Dr Keiller, who has resigned. Dr Brakenridge has also been appointed an Extra Physician in room of Dr Linton.

THE LORD CHANCELLOR has appointed Charles Lockhart Robertson, M.D. St And., F.R.C.P.E., etc., etc., to the office of Visitor in Lunacy, vacant by the death of Sir Charles Hood.

DR J. R. FRASER, Assistant to the Professor of Materia Medica, has been elected an Honorary Member of the Therapeutical Society of Paris.

JAMES SYME, Esq., D.C.L., etc., late Professor of Clinical Surgery, and JAMES HENDERSON, M.D.I.S., have been elected Honorary Fellows of the Faculty of Physicians and Surgeons, Glasgow.

THE LATE DR HERMANN VELTEN OF AIX-LA-CHAPELLE.—The medical profession in Britain, and the numerous invalids who have had occasion to resort to Aix-la-Chapelle, will hear with regret of the death of Dr Velten, which took place on 17th December last. Dr Velten had never recovered from the effects of an attack of Roman fever, which he had caught during a visit to Italy a year previously; and he ultimately sank under subacute pneumonia.

Dr Velten was a thoroughly-instructed and well-read physician, and kept himself quite *au courant* with the recent advances in practical medicine. He had paid special attention to the treatment of diseases of the skin, and had acquired a large experience among the many cases of cutaneous disorder which presented themselves among the visitors to Aix-la-Chapelle. To English patients he was very acceptable, from his thorough acquaintance with the English language, which he spoke, not only easily, but idiomatically. His knowledge of English, however, was not confined to its use as a means of intercourse with his patients. He knew and appreciated the literature, especially the lyric poetry, of the United Kingdom, and this he showed by a little volume of admirable translations into German verse, which he published in 1857, under the title of *Englische Gedichte übersetzt von Dr Hermann Velten.* In this volume will be found faithful renderings of poems by Campbell, Cowper, Burns, and Moore, besides the old Scottish ballad of "Sir Patrick Spence," two poems by Lady Flora Hastings, and Wolfe's "Burial of Sir John Moore." Not merely the sense, but the metre, of these is so well preserved that some of the songs, such as Burns's "Siller Tassie," can be sung as readily with the words of the translation as those of the original. The longest pieces in the volume are "Paradise and the Peri," and "The Light of the Harem" from "Lalla Rookh," and "Tam o' Shanter" from Burns. The latter is admirably rendered; and those of our readers who remember the beautiful episodical passage beginning,

"But pleasures are like poppies spread,"

and who can appreciate a bit of German, will readily accept the following as a sample of the fidelity of Dr Velten's translations:—

"Doch gleicht die Lust dem blüh'nden Mohn
Berührst du ihn, zerfällt er schon,
Sie gleicht dem Schnee, der in dem Fluss
Noch kaum gefallen, schmelzen muss,
Dem Nordlicht, dessen Strahlen man
Die flücht'gen, kaum erhaschen kann;
Des Regenbogens Farbenspiel,
Verschwindend vor des Sturms Gewühl.
Wer kann die Fluth, die Zeit wohl bannen!
Die Stunde kommt, Tam muss von dannen."

Dr Velten will be much missed by many who enjoyed the pleasure of his acquaintance, either as patients or professional brethren at Aix-la-Chapelle.

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Robert Barnes, M.D.,—Lectures on Obstetric Operations. London, 1870.

Braithwaite's Retrospect of Medicine,—July to Dec. 1869. London, 1870.

James L. Bryden, M.D.,—Report on the Cholera of 1866-68 in the Bengal Presidency. Calcutta, 1869.

F. Le Gros Clark, F.R.C.S.,—Lectures on the Principles of Surgical Diagnosis. London, 1870.

Prof. G. B. Crivelli e L. Maggi,—Sulla Coltivazione delle Forme Mieliniche.

Fifth Annual Report of the Sanitary Commissioner with the Government of India,—1868. Calcutta, 1869.

James Finlayson, M.D.,—On the Temperature of Children in Phthisis and Tuberculosis. Glasgow, 1869.

Austin Flint, jun., M.D.,—Physiology of Man. New York, 1870.

Douglas Galton, F.R.S.,—On the Construction of Hospitals. London, 1869.

Arthur Gamgee, M.D.,—On Force and Matter in relation to Organization. Edinburgh, 1869.

C. Haughton Gill,—Chemistry for Schools. London, 1869.

Michael C. Grabham, M.D.,—The Climate and Resources of Madeira. London, 1870.

Berkeley Hill, M.B.,—The Essentials of Bandaging. London, 1869.

T. Holmes, M.A.,—A System of Surgery. Vol. II. London, 1870.

H. Bence Jones, M.D.,—Life and Letters of Faraday. London, 1870.

J. L. C. Schroeder van der Kolk,—Pathology and Treatment of Mental Diseases. Translated by James T. Rudale. London, 1870.

Arthur Leared, M.D.,—Causes and Treatment of Imperfect Digestion. London, 1870.

William B. Lewis, M.D.,—The Pathology of Bright's Disease. New York, 1869.

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Frederick G. Snelling, M.D.,—Nocturnal Enuresis and Incontinence of Urine. New York, 1869.

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James Spence, F.R.S.E.,—Lectures on Surgery. Part II. Edinburgh, 1869.

Charles Squarey, M.B.,—On the Administration of Chloroform and Nitrous Oxide. London, 1869.

William D. Stone, M.D.,—Half-yearly Abstract of the Medical Sciences, July to Dec. 1869. London, 1870.

The Medical Directory for 1870. London, 1870.

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Duncan Turner, L.R.C.P.,—A Manual of Diet for the Invalid and Dyspeptic. London, 1869.

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Upsala Läkareförenings Förhandlingar,—Nos. 1 to 8.

Virchow's Archiv,—October, November.

# Part First.

## ORIGINAL COMMUNICATIONS.

### ARTICLE I.—*On Fatty Hernia.* By THOMAS ANNANDALE, F.R.S.E., Surgeon to the Edinburgh Royal Infirmary.

(*Read before the Medico-Chirurgical Society of Edinburgh, 5th January* 1870.)

GROWTHS of fat, in connexion with the various hernial regions, are met with under two principal conditions.[1]

1. As fatty tumours more or less circumscribed having no connexion with the peritoneum or sub-peritoneal fat;
2. As an increased development of the sub-peritoneal fat.

The first of these is by no means common, and has usually been met with in connexion with the inguinal, scrotal, or labial regions.[2] These tumours may simulate in their position and external appearance a true hernia, but are never attended by its more serious symptoms.

The second of these occurs most frequently as a growth or development of fat in connexion with a hernial sac, the fatty tissue being more or less adherent to the sac, and forming a covering to it, which is, however, not usually a complete one. This appearance is well known to all practical surgeons. More rarely, the fatty tissue or growth forms a complete covering or capsule round the sac, the neck of which may or may not be pervious. It is to these latter cases that I wish to direct attention, for their true nature has as yet attracted little notice, and is—with a few exceptions, to be presently considered—scarcely referred to by the authorities on hernia in this country.

In the year 1860, Professor Turner first directed my notice to some cases of this affection in the dissecting-room, and pointed out to me their anatomical relations. When Demonstrator of Anatomy in 1864, I met with two other well-marked cases, one of which I carefully dissected and noted at the time.

Professor Turner has kindly placed at my disposal his notes, giving an account of the dissection of the following four well-marked cases, and has also permitted me to give two illustrations copied from careful drawings which he had taken of Case 1:—

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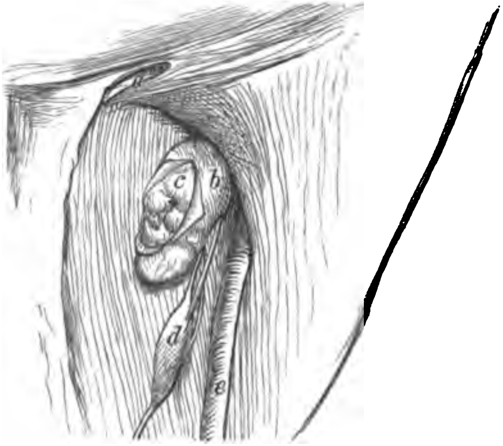
[1] Professor Wernher of Giessen has written an elaborate paper on this subject in Virchow's Archivs for July and August 1869. He considers these cases under the heads of Fatty Tumours Simulating Herniæ, and Fatty Herniæ.

[2] See Lawrence on Ruptures (fifth edition), page 253; Paget's Lectures on Surgical Pathology, edited by Turner, page 445; British Medical Journal for February 22, 1868.

Case 1.—A female subject, æt. 80, brought into the dissecting-rooms of the University in 1860. When the skin and superficial fascia were removed from the left groin, a large fatty mass was found projecting through the saphenous opening. It was 1¾ inches long and 1 inch broad at its widest part. It was situated on the pubic portion of the fascia lata, somewhat constricted at the spot where it projected through the saphenous opening, being closely embraced on its outer surface by the superior cornu of that aperture. When the fascia lata was removed and the crural sheath exposed, and the different compartments cut into, the internal or crural canal was found to contain a considerable quantity of fat, which was continuous with the fatty mass projecting through the saphenous opening. In its descent the mass had projected before it that part of the femoral sheath which forms the wall of the canal. *This investment* (*b*, Fig. 1) *had a thin transparent shining appearance which might, at first sight, be mistaken for the sac of a hernia.* When this investing structure was cut through, the fatty contents (*c*, Fig. 1) were found to be distinctly lobular and vascular, the vessels being continuous with the vessels of the abdominal wall. The lymphatic vessels were situated both at the saphenous opening and in the crural canal behind the fatty mass. A funnel-shaped peritoneal sac (*e*, Fig. 2) was situated in the middle of the fatty mass, extending for about half an inch downwards. The fat was evidently nothing more than a hypertrophy of the sub-peritoneal fat of the wall of the abdomen, with which it was continuous, superiorly through

Fig. 1.

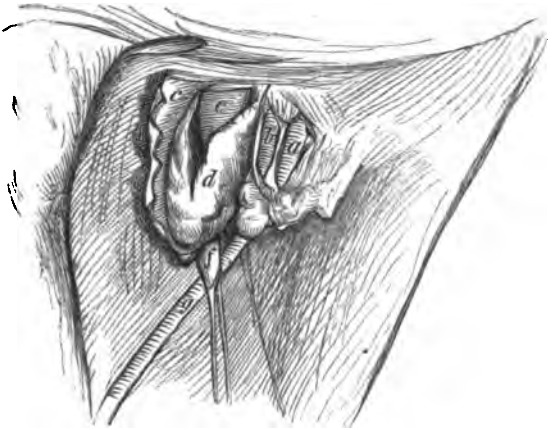


SUPERFICIAL DISSECTION OF CASE I.

*a*, The external abdominal ring.
*b*, The crural sheath, which has been laid open to expose the fatty mass, *c*.
*d*, A lymphatic gland.
*e*, The internal saphenous vein.

the crural canal. The peritoneal sac was smooth and transparent. The sac was empty, and its upper narrow end communicated with the general cavity of the peritoneum. The fatty mass was slightly adherent to the exterior of the sac. In this same subject a fatty mass projected through the right external abdominal ring. It was about the size of an almond, and was covered by a fibrous investment. At first it appeared as if situated in the tissue of the round ligament, but on more careful examination it was found to be quite distinct from it. It consisted of a mass of sub-peritoneal fat, which had come through the abdominal wall in the position of the direct hernia, pushing before it the fascia transversalis, breaking through the fibres of the conjoined tendon, the spermatic fascia being prolonged in front of it. There was no sac in its interior, as the parietal layer of the peritoneum was quite smooth, and free from depression.

Fig. 2.



DEEPER DISSECTION OF CASE I.

*a*, The femoral artery.
*b*, The femoral vein.
*c*, The cut margin of the crural sheath.
*d*, The fatty mass, which has been opened in order to show the peritoneal sac *e* in its interior.
*f*, A lymphatic gland.
*g*, The saphenous vein.

CASE 2.—A female subject brought into the dissecting-room in June 1860. In this case a mass of fat, the size of a small orange, projected through the left saphenous opening, and was covered by the femoral sheath. An incision made into the centre of this mass of fat exposed a peritoneal sac containing omentum, which was adherent to the inner surface of the sac. On the right side a fatty mass, the size of an almond, and having the same anatomical relations, protruded through the saphenous opening. An incision into the fatty mass showed a small peritoneal sac, which was empty.

CASE 3.—A female subject, dissected in July 1860. On opening into the left crural canal there was found a lump of fat the size of a large olive. It was continuous with the sub-peritoneal fat around the crural ring. When carefully cut into, a tube of thickened peritoneum was found in its interior. The tube was empty, and extended to within a quarter of an inch of the lower end of the fatty mass, where it ended in a cul-de-sac. It was closed in superiorly by a distinct cicatrix, especially seen when the inner surface of the peritoneum was examined.

CASE 4.—A female subject, dissected January 1861. A mass of fat, one inch in length, protruded through the left saphenous opening. It was continuous with the sub-peritoneal fat. When cut into, a peritoneal sac was found in its interior. The sac was empty.

The following case is one which I myself dissected:—

CASE 5.—A male subject, middle-aged, dissected in March 1864. On reflecting the skin, fascia, and external oblique muscle on the

Fig. 3.

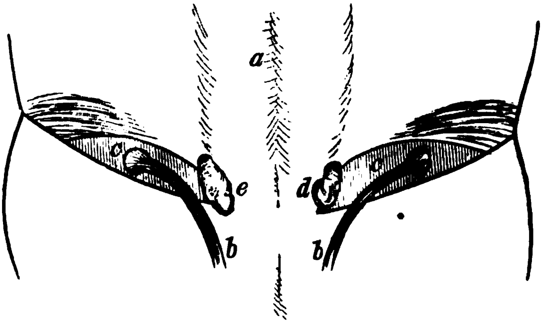


Fig. 4.

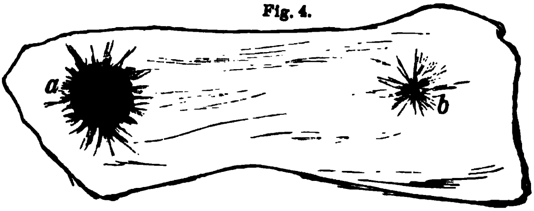


FIG. 3.—DIAGRAMMATIC SKETCH OF CASE 5.

*a*, The linea alba. *b*, *b*, The spermatic cords.
*c*, *c*, The transversalis fascia. *d*, The left fatty hernia.
*e*, The right fatty hernia.

FIG. 4.—APPEARANCE OF CASE 5 FROM THE INNER ASPECT OF THE PERITONEUM.

*a*, The opening into the peritoneal sac on the right side.
*b*, The opening into the peritoneal sac on the left side.

right side, a fatty mass (*e*, Fig. 3), the size of a pigeon's egg, was exposed. It protruded in the situation of a direct inguinal hernia, having pierced the fibres of the conjoined tendon. On laying open the mass a distinct peritoneal sac was found in its interior. The sac was empty, but communicated superiorly with the general peritoneal cavity by an opening (*a*, Fig. 4) large enough to admit a finger. The margins of the opening, as seen from the inner wall of the abdomen, presented a puckered appearance.

On the left side there was a similar mass of fat (*d*, Fig. 3) not quite so large, but having the same anatomical relations. It also contained a peritoneal sac in its interior, which communicated with the general peritoneal cavity superiorly by an opening (*b*, Fig. 4) the size of a small quill. The margins of this opening presented the condition of a cicatrix, and the peritoneum was very puckered for some little distance round.

I am indebted to Dr Chiene for an opportunity of examining the next case.

CASE 6.—A male subject, dissected December 1869. On the right side there was a large oblique inguinal hernia, which had passed down into the scrotum. A lobulated fatty mass projected in the situation of a direct inguinal hernia on the same side. This mass of fat, when opened into, was found to contain a distinct peritoneal sac, which, however, did not communicate with the general peritoneal cavity; but there was a well-marked cicatrix on the inner wall of the abdomen immediately opposite the situation of the protrusion. In the right crural region a mass of lobulated fat projected. It was continuous with the sub-peritoneal fat, but did not contain any sac. On the left side there was an ordinary crural rupture, which had some fat connected with its sac, but this fat did not surround or invest the entire sac as in the direct inguinal protrusion.

Sir A. Cooper[1] met with a case in which there was a "steatomatous" tumour, occupying exactly the situation of a crural hernia. It appeared that the patient, a woman, had suffered from crural hernia, that the sac had gradually contracted, for a very small portion of it remained in the crural orifice, and that the space had not only been occupied by fat, but that this had grown into a tumour of considerable size.

Meckel[2] refers to the protrusion of fatty hernia through the abdominal walls, and says this form of hernia is rare. Fardeau[3] met with the case of a man, æt. 65, who had three fatty herniæ, one below the ensiform cartilage, one at the umbilicus, and a third in the neighbourhood of the left spermatic cord. Morgagni, Pelletan, and Cloquet observed cases of this kind, in some of which there was a small empty peritoneal sac.[4] Mr Cock[5] relates the case of a married woman, æt. 27, who had a very small tumour in the left crural ring, irreducible and very tender to the touch. It had existed nine months, but had not given rise to any inconvenience. As symptoms of obstruction were present, Mr Cock operated, and exposed a hard lobule of fat, which, when turned aside, showed a small hernial sac empty. Mr Cock also refers to two other cases, in which a small protrusion existed at the linea alba. These protrusions were tender to the touch, and occasionally gave rise to painful symptoms. Mr Cock considered that they consisted of sub-peritoneal fat.

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[1] Cooper on Hernia.

[2] Handbuch der Pathologische Anatomie, 1812.

[3] Sedillot's Journal de Médecine, vol. xviii.

[4] Laurence, *loc. cit.*

[5] Med. Chir. Transactions, vol. vii.

I lately saw, with Dr Chiene, a woman who suffered from a fatty protrusion of this kind. It was situated at the outer edge of the rectus muscle, was the size of a small mushroom, and gave rise to very painful symptoms. It remained protruded for some weeks, and was then returned into the abdominal cavity.

Two other examples of this form of hernia, afterwards to be described, have occurred in my practice during the last few years, but their true nature was only determined after operating upon them to relieve symptoms of strangulation.

The kind of hernia under consideration consists, then, of a mass of fat more or less lobulated, continuous with the sub-peritoneal fat, and forming a complete closely investing capsule round a true peritoneal sac, which may or may not be shut off from the general peritoneal cavity.

It would appear, from the dissection of some of the cases referred to, that masses of sub-peritoneal fat occasionally protrude without having any peritoneum in their interior. It has been found, in connexion with the inguinal and crural regions, the linea alba, and linea semilunaris; but it is probable that it may occasionally occur in any of the other hernial regions.

The size of such herniæ varies from a small orange to an almond. They may exist singly, or be present in more than one situation in the same patient; or they may be present when other forms of hernia exist. They may be reducible, but are, perhaps, more frequently irreducible, especially when they have existed for some time; this latter condition being owing partly to adhesions, and partly to the increased growth of fat which may take place while the protruding structures are in their abnormal position. They may also become strangulated, and give rise to the usual signs of this condition if the sac in their interior remains sufficiently open and communicates with the general peritoneal cavity, so as to allow the passage into it of some of the important abdominal contents. The development of fat in some of these cases would appear to take place in connexion with a true hernial sac which has become shut off from the general peritoneal cavity, or which is undergoing a spontaneous contraction. In other cases, Mr Quain considers that the protrusion of the fatty mass is the first condition, a process or tube of peritoneum being gradually drawn down into it.[1] There are, however, three pathological facts important to the practical surgeon, to be noted in the consideration of these herniæ:—

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[1] Quain on Hernia, Medical Times and Gazette for January 6, 1855.

(1.) That the fatty mass composing the bulk of the hernia may contain a sac in its interior, which communicates with the general peritoneal cavity, and permits portions of omentum or gut, or both, to pass down into it.

(2.) That the fatty mass may be invested by a covering, such as the femoral sheath, as noted by Professor Turner in Case 1, and by myself in Cases 7 and 8; or some fascia which simulates a true hernial sac, and might, in the case of an operation for strangulation, mislead the surgeon, and induce him to return the fatty mass without discovering the true sac in its interior.

(3.) That several of these herniæ may exist in the same patient, one or more of which may contain pervious sacs, while another, or others, may have no sac in their interior, or have one which had become shut off from the general peritoneal cavity; and that a patient may have one or more of the common forms of hernia, in addition to one or more of these fatty herniæ.

*Symptoms.*—Having had only a limited experience of these herniæ in the living subject, I cannot pretend to describe any special symptoms which may assist in their correct diagnosis. A knowledge of their pathology would, however, lead us to expect that they will be found in some of the usual hernial regions, and that their consistence will be firmer and more solid than that of an ordinary hernia. The protrusion of such herniæ may give rise to no symptoms except the presence of a swelling, or they may cause pain and signs of general uneasiness in the abdomen without the more serious symptoms of strangulated hernia. When they contain a hernial sac with a portion of gut or omentum in its interior, the usual symptoms of strangulated hernia may at any time present themselves. Cases in which cysts or serous cavities have obscured or simulated a femoral hernia have been reported by Mr Quain[1] and Mr Heath.[2]

*Treatment.*—Professor Wernher expresses the opinion that the development of fat in some of these cases may be useful by acting as a plug, and preventing further protrusion, so that if the hernia is giving rise to no uneasiness and is irreducible, it will be best left alone. If painful symptoms alone are present, rest, and the application of cold, will probably relieve them.

The account of the dissections, and the history of the cases to be now related, show how important it is that masses or lobules of fat —exposed by operation in any case of strangulated hernia—should be carefully searched, lest they contain a sac and contents in their interior. If the fatty mass cannot be returned after the stricture has been divided, and the sac carefully examined, and any contents pushed back into the abdomen, it may, I think, be advantageous to apply a ligature round the neck of the sac, so as to endeavour to shut it off from the general peritoneal cavity, and, at the same time, to leave the fat instead of cutting it off.

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[1] Medical Times and Gazette for January 6, 1855.

[2] Do. September 7, 1861.

If the symptoms of strangulated hernia should be well marked, and the exposure of such a hernia by operation does not detect any constricted sac with contents, the possibility of another hernia existing, and causing the symptoms, must be taken into careful consideration. The following two cases are very illustrative examples of this kind of hernia, observed in the living subject:—

CASE 7.—In June 1867, I was asked by Dr Burn to see Mr F., æt. 65, who was suffering from the symptoms of strangulated hernia. The patient had suffered for thirty years from a femoral hernia on the left side, which had been irreducible for many years. He had also a femoral hernia on the right side, which, however, was easily reducible. Three days before I saw him the symptoms of strangulation had commenced, and he had been vomiting fæcal matter for the last twenty-four hours. After a careful examination it was evident that the strangulation affected the hernia on the left side, and I accordingly proceeded to cut down upon the tumour in the usual way. Having dissected off the skin and several other layers, I exposed a transparent shining membrane, like that referred to in Case 1, as so closely resembling the sac of a hernia. When this was opened a mass of lobulated fat, the size of a pigeon's egg, was exposed. This mass had a distinct neck passing down from the abdominal cavity, and was tightly grasped in the region of Gimbernat's ligament. Thinking this fatty mass to be a portion of omentum, I divided Gimbernat's ligament, and proceeded to reduce the hernia. When pressing it up I felt the distinct gurgle of a portion of gut returning into the abdominal cavity, but I could not succeed in reducing the greater part of the mass. Still looking upon this fat as a piece of irreducible omentum, I tied a ligature round its base, and then cut it off outside the ligature. On examining the piece of omentum removed, I was startled to find in its interior what at first sight looked exactly like a piece of intestine which had been cut off along with the fat. A more careful examination, however, showed that this was merely a portion of the sac which had been situated in the interior of the fatty mass, and which had contained a portion of gut. The symptoms of strangulation were at once relieved by the operation, and the patient made a good recovery, and is still alive and well.

CASE 8.—In June 1869, I was asked by Dr Niven to see Mrs ——, æt. 78, who was suffering from symptoms of strangulated hernia. The patient had observed a small lump in the right groin for many years, which was not reducible, but it did not trouble her. Three days before I saw her, after making a sudden slip with her right leg, she felt pain in the tumour, and was seen next day by Dr Niven, who found her suffering from symptoms of strangulation, which were not, however, very severe. As these symptoms were much aggravated the following day, I was asked to visit her.

An examination showed a firm swelling, the size of a walnut, in the region of a femoral hernia on the right side. The tumour was very tender to the touch, and completely irreducible. As the symptoms of strangulation were well marked, I cut down upon the swelling, and exposed a mass of lobulated fat, contained, as I thought at first sight, in a true sac. The mass of fat had a distinct neck passing down from the abdominal cavity, and firmly grasped at the usual seat of stricture. Having divided Gimbernat's ligament, I was just upon the point of returning the mass—thinking it to be omentum—when I remembered my former case, and also the dissections shown me by Mr Turner. Before, therefore, pushing back the mass, I carefully opened up the fat, and was rewarded by finding, in its very centre, a small true sac, which, when opened, contained a knuckle of congested gut. On attempting to return this piece of gut, I found it would not go up, and on passing my finger up to the neck of the sac, I discovered that the intestine was constricted by it. A more free division of the sac and its neck was made, and then the gut was easily pushed back into the abdomen. This patient also made a good recovery, and still remains alive and well.

*Remarks.*—These two cases require little comment from me, as they will, I think, speak for themselves. In the first case, it was fortunate that I had returned the piece of gut contained in the sac before cutting off the fatty mass mistaken for omentum, otherwise the gut could scarcely have escaped serious injury. In the second case, it was also fortunate that the fatty mass was opened up and the true sac discovered, for if this mass had been reduced whole, the patient could have received no benefit from the operation, as there was a distinct stricture at the neck of the sac.

## ARTICLE II.—*On Galvano-puncture of Nævus.* By JOHN DUNCAN, M.A., M.D., etc., Assistant-Surgeon to the Royal Infirmary of Edinburgh.

I HAVE already published in the *Edinburgh Medical Journal* (1866 and 1867) two papers on the use of electrolysis in aneurism, and I related among others a severe case of the cirsoid variety cured by that means. The resemblance in structure between cirsoid aneurism and nævus encouraged me to hope that electrolysis would be as successful in the latter as it had been in the former. It is impossible as yet to ascertain the exact value of this form of galvanism in the treatment of blood-tumours; but the cases I here shortly note may prove of use in determining how far it is applicable to nævi.

CASE 1.—The nævus occupied the greater part of the upper eyelid of a child 8 months old, and was rapidly increasing in size. After several operations it was almost entirely hardened, but in consequence of the impatience of the parents the case was completed by subcutaneous ligature.

In CASE 2, the nævus occupied the upper eyelid of a child 6 months old, and was about the size of a walnut. The skin was implicated only in so far as it had one or two visible veins coursing over the tumour. After five operations the whole was hardened, and greatly diminished in size. As the parents were obliged to return to the country, I lost sight of the child for some time; but I lately heard from them that it died of bronchitis some months after the last operation, and that then the swelling had entirely disappeared.

CASE 3 was a nævus of the forehead, implicating the skin that covered it, and was sent me by Sir James Simpson. The child, a year old, was brought from the country by its parents, who took it back immediately after one operation had been performed. The electric action was continued till the skin assumed the whitish hue that indicates cauterization. The child was to be brought back for a second operation if the cure was not complete; but it has not since been heard of. I have no doubt that it was cured.

CASE 4 was a nævus of the upper eyelid, very similar to Case 1, but with the skin completely involved. Two operations hardened the subcutaneous portion, which was thereafter largely absorbed. After two months, as it was again increasing, an operation was directed to the cure of the skin, which had not as yet been affected by the electrolysis. Its vitality was destroyed, and a black slough separated almost without suppuration. A cicatrix remained, but did not interfere with the motions of the eyelid.

CASE 5.—A nævus of the abdomen, of small size, cutaneous and subcutaneous, in a boy 5 years old. A slough separated after one operation, which effected the cure.

CASE 6 was a nævus on the forehead of a child 11 months old, sent me by Dr Aitken. It was very flat, from the skin being more involved than the deeper textures. It was cured in three operations, two of which produced small sloughs. I embraced the nævus in a ligature, to arrest the blood-current, while one of the operations was being performed; but this proceeding did not apparently affect the result.

I have since successfully treated one or two other nævi in the same way, but as they do not materially differ from those already mentioned, they need not be further detailed. Some were simply hardened, and thereafter became absorbed; others had more or less sloughing, according to the amount of nævous skin.

There can, I think, be no doubt that any nævus is curable by electrolysis. From the result of these cases, however, I fear that its practical usefulness is limited, and that many nævi can be more quickly and conveniently cured by other means. There is, from their very nature, this marked distinction between cirsoid aneurism and nævus in amenability to treatment—that, while in the former the blocking-up of main affluents leads to the extinction of the rest of the tumour; in the latter, occlusion of a veinule seems to have little influence on its neighbours. Nævi have this obstinate characteristic, whatever mode of treatment be adopted. If a portion be left out when the ligature is applied, its growth is accelerated rather than otherwise. When the subcutaneous part of the tumour is cut out, according to Mr Jordan's recommendation, it would seem that the affected skin over it remains unchanged. Perchloride of iron must by its immediate presence produce a clot in each individual vessel before the cure is complete. In like manner, the electrolytic action must pervade the whole nævus, and act directly on almost every little vein. Now, electrolysis cauterizes the tissues as much as it coagulates the blood; and it is evident, accordingly, that when the skin is much affected a slough will be produced. Several of the cases are examples of this. The slough is tough and dry, excites little irritation, is singularly slow to separate, and does so without a drop of hæmorrhage, even though part of the nævus remains. Galvanism is safe therefore and effectual; but if a cicatrix must be left, there is reason for preferring the ligature as more simple and speedy. The galvanic apparatus is cumbrous, and in most cases the operation requires frequent repetition. Still, if the skin be not greatly occupied by the enlarged vessels, sloughing may be avoided by a very cautious approach to the surface, and by using small needles and a weak current. In subcutaneous nævi, moreover, especially when they occur about the head and neck, galvano-puncture possesses many advantages. The injection of such astringents as perchloride of iron is both less safe and less certain; and excision necessarily leaves a scar, which electrolysis never does, if properly insulated needles be employed. Again, there is a class of nævi which lie beyond the reach of knife and ligature. Some months ago Mr Spence asked my opinion in a case of nævus, which so occupied the hand of a child (a year old) that the fingers protruded from the mass like small roots from a turnip. He told me that his experience of injection in a similar case was such that he should prefer amputation. My opinion was that a cure could be brought about by galvanism, and probably without loss of skin. The result has amply justified the opinion. After two or three operations in the Infirmary, the patient was taken home to Peebles, where Dr Connel so carefully and skilfully continued the treatment, that when the child returned, a few days ago, hardly a trace of the nævus remained. A few hard nodules occupied the palm, but the motions of the hand are perfect. The only accident had been a slight suppuration after one of the operations. There is also at present in the Infirmary, under Mr Spence's care, a girl about 9 years old, whose left cheek is wholly occupied by a large nævus, which extends over the parotid region and part of the neck. I have already aided in the performance of two operations which have hardened a large portion of the cheek, and I have no doubt that the treatment will be successful. It is a case of greater difficulty than the other, because, especially in the neck, the skin is traversed in many places by a network of vessels, large and small. The subcutaneous part of the nævus bears, however, a very large proportion to the tegumentary; and I think it likely that there will be no sloughing or consequent scar.

These two cases illustrate well the class of nævi for which galvanism is best suited. Indeed, its only drawback is that it demands considerable patience on the part both of surgeon and patient. In every other respect it is superior to injection, ligature, and excision. Mr Teale, it is true, has recently advocated and put in practice excision of nævus of the parotid. But in the case I have described, the extent of the nævus plainly forbade such an attempt; and even had it been smaller, a treatment so easy and safe as galvano-puncture must be preferred to excision.

I have nothing to add to my former descriptions[1] of the apparatus used. Other batteries may be more convenient than Bunsen's, but, having become familiar with its manipulation, I have not thought it worth while to change. The needles I formerly recommended have invariably been found to answer the purpose completely.

Before concluding, let me say one word on an objection to electrolysis, which has been made a good deal of by various recent writers —I mean the introduction of gas into the circulation. Professor Rutherford,[2] in his able article on Electro-therapeutics, recommends that the positive pole be alone introduced, as its gaseous products are more easily absorbed, and it causes the firmer coagulation of blood. I might be inclined to follow his advice in a case where one needle only is required, if such a case may be met with. But had he observed the effect of the negative pole, as I did notably in the cirsoid aneurism, and had he remembered that that needle is much the more powerful cauterizing agent, I believe he would have modified his recommendation. It is useful not only in coagulating the blood, but also in shrivelling-up and destroying the coats of the vessels through which it passes. In such cases, therefore, I strongly advocate the introductiou of both poles, for I hold that the presence of gas in the circulation is of very slight importance. Mr Lawson Tait,[3] indeed, says that he "assisted to kill" a patient with innominate aneurism by this means, but he gives no details, and he is the only man who has done so. I can well believe that in innominate aneurism, if the carotid have not been compressed, clots and froth and everything may have been whirled from the needles to the brain as quickly as they were formed, and that fatal embolism might result. But those who have read the researches, especially of French surgeons, into the causes of death by the introduction of air, will see at once that this source of danger is not present in galvano-puncture. It has its dangers, which I believe render it not very suitable for aneurisms like the innominate, and these I have pointed out in a former paper; but for cirsoid aneurism, for some not amenable to compression, and probably for such as the gluteal and sciatic, it is an admirable mode of treatment, and I would hope that the cases I have related may in some measure determine its use in a certain class of nævi. It is hardly necessary to add, that the spontaneous disappearance of nævi is so frequent as to render the number suitable for any operation comparatively small.

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[1] Edinburgh Medical Journal, April 1866 and August 1867.

[2] itish Medical Journal, 1868.

nham Society Year-Book, 1868.

## ARTICLE III.—*Case of Diphtheritic Ophthalmia.* By D. ARGYLL ROBERTSON, M.D., F.R.C.S.E., Lecturer on Diseases of the Eye.

(*Read before the Edinburgh Medico-Chirurgical Society, 2d February* 1870.)

JOHN MACFARLANE, a delicate child, of pale livid complexion, two years of age, was brought to the Eye Dispensary on the 20th of October last, on account of severe inflammation of the left eye, which had existed for two or three weeks. Beyond bathing of the eyes with milk-and-water, little treatment had been adopted. The mother stated that none of the neighbours' children had sore eyes, nor was she aware that the inflammation had been communicated to her child in any way. The little patient had never been very strong, and of late had lost its appetite, and was hot, feverish, and restless. It had previously suffered from severe inflammation in its eyes when about five months old.

On examination, the lids of the left eye were found of a dusky red colour, and swollen, presenting much the appearance of a case of purulent ophthalmia, and the skin of the lower lid and cheek excoriated. On examining the eye, a very distinct light yellow membrane was found coating the conjunctivæ of both lids, and the palpebral margins were slightly ulcerated and covered with a similar membrane. The membrane was tough, and pretty firmly adherent to the conjunctiva, but could be removed in shreds, leaving a smooth bleeding surface; but an interval of one or two days always sufficed for its reproduction, and, however carefully it might be removed at one visit, it was found to have reappeared at the next, two days later. The secretion was watery, and of a light-gray colour, and the lids were thickened from the effusion of fi y
exudation, which exudation had so altered their character tha ered
could not be readily everted. In these respects the case ck and
from one of purulent ophthalmia, in which the secretion i everted.
of a deep yellow colour, and the lids are very ea

At first the conjunctiva of the globe was slightly injected, but not covered with membrane, and the cornea was quite clear; but at the next visit, two days later, the ocular as well as palpebral conjunctiva was found covered with membrane, and at the end of a week the lower part of the cornea appeared dim, and of a grayish colour. A fortnight later, the membrane had entirely disappeared, a small portion of the cornea had sloughed away, and the eye presented exactly the appearance of one recovering from an attack of purulent ophthalmia. The inflammation subsided in the course of four or five weeks, and when the patient was last presented, there was little beyond a slight opacity of the lower portion of the cornea, to indicate the severe inflammation from which it had suffered. In the right eye the conjunctiva was also slightly inflamed, but at no time presented the least appearance of membranous formation. I noticed that the neck was swollen, which, upon reflection, I am inclined to attribute to glandular infiltration.

The treatment at first consisted in the enforcement of rigid cleanliness, the secretion being removed every half-hour, or oftener if necessary, and the use of a weak astringent alum wash. When the patient was brought to the Dispensary, the membrane was removed, and the bared surface pencilled over with a solution of nitrate of silver (gr. x. to ℥j.). The mother was cautioned not to allow any of the secretion to touch the other eye, and to make the boy sleep on his left side. Little benefit appeared to result from the treatment adopted, so at the end of a week the alum wash was stopped, and a weak solution of permanganate of potass (1 part of Condy to 7 of water) substituted; and as the cornea became implicated, atropine was applied at the Dispensary visit in place of the nitrate of silver. In the later stage of the disease quinine was administered as a tonic.

I have been induced to record this case chiefly on account of its rarity; for though epidemics of diphtheritic ophthalmia have been reported as occurring at Berlin and other parts of the Continent, only one or two cases have been observed in Great Britain. But though no epidemic has yet occurred in this country, it is eminently desirable that we should be aware of the fact that such may occur, so that the profession should not be found unprepared to treat it if it should arise.

It may be well, before considering the features of the present case, to state briefly the views entertained of this disease by Continental oculists, who have had opportunities of observing epidemics. The first distinct account of the affection we owe to Professor Von Graefe, who narrated his experience of epidemics he had witnessed in Berlin in 1852 and 1853, and to whose observations little has [illegible] added by subsequent writers. He observed, that in these cases [illegible]inous exudation was poured out into the substance of the [illegible]iva, and upon its surface forming a tough yellow membrane [illegible] adherent to the conjunctiva. From the nature of the exudation the lids were stiff, and could not be easily everted. The bloodvessels of the conjunctiva were in the early stages of the disease here and there distended, and some extravasations of blood might be observed, which he, however, attributed to mechanical obstruction, due to the firm infiltration of the subconjunctival tissues. As the disease advanced the conjunctiva became pale and yellow, and incisions failed to evacuate either blood or exudation. The secretion was thin, and of a dirty gray colour, with a few yellow flakes floating in it. The local increase of temperature, and the pain felt, were always more severe than in cases of purulent ophthalmia. After a variable time, he observed marked changes to occur in the characters of the disease—the lids lost their stiffness, the conjunctiva became more vascular, and presented a spongy appearance, while patches of membrane were thrown off from its surface, and injected papillary elevations were developed. In the last stage, the disease presented all the appearance and symptoms of an eye recovering from purulent ophthalmia, except that when fibrous exudation into the conjunctiva had occurred to a great extent, as the inflammatory symptoms subsided, cicatrices formed, and a degree of shrinking of the lids occurred. At the height of the disease, the cornea was apt to suffer from the defective circulation, and in this the chief danger of the disease lay; a slough formed, which tended to advance and involve the greater part of the cornea, leading to perforation, prolapse of the iris, and the formation of a dense staphyloma.

This form of conjunctivitis usually attacked persons in feeble health, and during the inflammation, or subsequent to it, affections of internal organs frequently manifested themselves. Thus, in 40 cases of diphtheritic conjunctivitis in children, Von Graefe observed fatal results—in 3 cases from croup, in several from pneumonia, and in others from hydrocephalus. Very often diphtheritic inflammation of the skin of the nostrils, of the angles of the mouth, or at places where blisters had been previously applied, was observed as a complication. The chief cause of the malady is an epidemic one. The main outbreaks appeared in spring and autumn, and at these times many other diphtheritic affections prevailed, particularly of the respiratory passages, and also puerperal fever, while almost all other severe conjunctival inflammations exhibited a certain tendency to the diphtheritic form, in this, that the swelling was firmer, and the exudation richer in solid material. Most of the patients were between two and three years of age. Von Graefe found that the secretion was distinctly contagious, but, curiously enough, while in some cases inoculation excited a similar inflammation, in most it appeared to induce a simple conjunctivitis, attended with purulent or muco-purulent secretion (blennorrhœa). As indicative of the severity of the disease, in one-third of the cases the eyes were completely lost. Von Graefe found the severity of the individual cases proportionate to the extent of the exudation. In this disease, as in purulent ophthalmia, it was observed that any previous affection of the cornea, attended with vascularity, exerted a favourable effect on the course of the disease. He also ascertained that the type of disease was always most severe at the commencement of the epidemic; and that, while in the severe cases well-marked cicatrices were observed to result, in the milder a scarcely recognisable shrinking of the conjunctiva alone occurred. Von Graefe observed many mixed forms of the disease, presenting symptoms more or less allied to the diphtheritic and purulent inflammations.

It appears to me to be a question of much interest and some practical moment to consider whether the disease under discussion is truly diphtheritic or not. Considerable confusion appears to prevail in regard to the true nature of diphtheritic and diphtheroid diseases. In the more ordinary situation three varieties of such affections occur—namely, the simply parasitic, composed mainly of the oidium albicans; the inflammatory or croupous; and the truly diphtheritic.

It is true that structurally there is no difference between the second and the third varieties, both presenting under the microscope exactly similar appearances, and the distinction that some have sought to establish according to the state of the tissues beneath the membrane (namely, whether ulcerated or not) is equally unreliable; but the clinical history of the diseases renders it evident that the processes are essentially distinct, the one presenting all the symptoms of a severe local inflammation, the other being also inflammatory, but dependent upon a constitutional blood-poisoning. Now, is the disease described truly diphtheritic? We may, I think, at once dismiss the idea of its being parasitic or allied to thrush. What are the characters by which we distinguish the two other diseases? In the simple inflammatory form the constitution suffers from the local disease; in the diphtheritic, the local disease arises from the constitutional affection. The simple inflammatory is not epidemic, the diphtheritic generally is. The inflammatory is not contagious, the diphtheritic is. This last characteristic, however, does not hold good when the conjunctiva is the seat of the disease, as then both varieties are contagious. Dr Millard appears to have been the first to direct attention to the condition of the blood in diphtheria. He found that its colour is altered from red to brown, comparing it to the juice of plums or liquorice, while Dr Peter compares it to water coloured with soot. These observations have been confirmed by Trousseau. In the so-called diphtheritic conjunctivitis, there is one other diagnostic symptom on which considerable stress is laid—namely, the infiltration of the conjunctival and subconjunctival tissue with a dense fibrinous material, marking it off from simple inflammatory conditions in which the effusion is fluid and serous; but this obviously does not necessarily imply that it is diphtheritic in its nature. The points which favour the diphtheritic view are—its occurrence as an epidemic, accompanied by and associated with other diphtheritic complaints, its occasional inoculability, and, what is also of no little importance, that in a large proportion of the cases death ensued. Now, no conjunctival inflammation of itself proves fatal, and if patients suffering from it as a main local symptom die, this is surely an indication of some constitutional malady. If we further take into consideration that during these epidemics many cases of simple conjunctivitis are found to take on a similar unhealthy action, I think we cannot avoid concluding that the disease does in truth occur as a local manifestation of a blood-poisoning. Stellwag von Carion and some other authors refer to a membranous form of conjunctivitis, but this, I think, finds its place among those mixed forms which Von Graefe has so clearly described.

Taking these points into consideration, I am inclined to consider the case I have narrated as in all probability a true example of diphtheritic ophthalmia, although the symptoms were not in all respects characteristic. The child's appearance was highly suggestive of blood-poisoning, and such exhaustion and debility were occasionally manifested during the process of examination after the disease had reached its height that syncope was feared. The local lesion presented all the characters described by Von Graefe, the chief peculiarity being the limitation of the disease to one eye.

The treatment pursued by Von Graefe in the early stages of the disease consisted in the assiduous application of ice-compresses, deep local scarification, leeching, and the internal administration or more generally the inunction of mercury. When the acute stage had passed, the conjunctiva was frequently touched with mitigated caustic, and the ice-compresses less frequently employed. Von Graefe found that the application of caustic in the *acute* stage served rather to aggravate the disease. When the cornea became markedly affected, recourse was had to paracentesis. This, with great attention to cleanliness, constituted the treatment.

In the case I have recorded, I did not consider the administration of mercury nor local depletion would prove serviceable, the patient being feeble and exhausted. I therefore contented myself at the first in enjoining cleanliness, and the use of a weak astringent wash, which I soon changed for a solution of permanganate of potass, which latter appeared to produce very beneficial effects. The only constitutional treatment adopted was the use of quinine as a tonic after the severity of the disease had passed. The result obtained was much more satisfactory than I had reason to anticipate.

## ARTICLE IV.—*Case of Sloughing Hernia.* By PATRICK HERON WATSON, M.D., F.R.S., F.R.C.S.; Lecturer on Surgery; Surgeon to the Royal Infirmary and Chalmers Hospital.

(*Read before the Edinburgh Medico-Chirurgical Society, 2d February* 1870.)

MRS R., æt. 50, was admitted to the Chalmers Hospital in the course of the evening of Saturday, 11th December 1869, suffering from a femoral hernia upon the right side.

She states that the hernia came down upon Tuesday the 7th, since which time it had resisted every effort to effect its reduction. Her bowels also had been constipated. There had, however, been no severe pain or vomiting till the morning of the day of her admission, when pain in the tumour, extending to the umbilicus, and repeated efforts at retching, had set in.

On examination by my house surgeon, Dr Scott, the tumour was found to be tense, and tender on manipulation; but as there was neither vomiting nor abdominal pain, he deemed it unnecessary to send for me, and applied warm fomentations.

On Sunday, Monday, and Tuesday, the symptoms continued much as before, the patient feeling so well that she was anxious to return home. The pain in the tumour was only elicited upon firm pressure; there had been no vomiting, but the constipation was unrelieved.

*Wednesday, 15th December.*—There is more tenderness about the tumour, and the skin seems to glide less readily over the surface. There is also slight febrile disturbance, but no vomiting.

I deemed it, in such circumstances, most desirable to cut down upon the tumour without further delay. In doing so, the soft parts beneath the skin were found closely agglutinated to each other and to the sac. On opening the sac, a quantity of very fœtid sero-purulent fluid escaped; and on freely opening up the sac, the knuckle of bowel constituting its only content was exposed, presenting an ash-gray colour, a distended appearance, and a dulled lustre. There was no lymph adherent to either the sac or intestine, except at the very neck, and the consistence of the bowel was such as to forbid any further manipulation than could be avoided. I accordingly contented myself with dividing the stricture in the ordinary situation, by means of an ordinary curved probe-pointed bistoury. After washing out the sac with water, a morsel of wet lint was inserted to prevent the agglutination of the margins of the incision; a pad of lint, likewise wet, was then applied, and covered in by means of a piece of gutta percha tissue. An opiate was administered so soon as she had emerged from the chloroform. Diet to consist of milk and beef-tea. The opiate to be repeated as occasion may direct.

*22d December* (7 days).—During the past week the dressing has been changed daily, and has consisted simply of lint soaked in water. To-day the whole of the peritoneal coat of the bowel, along with some portion of the sac, have separated in the form of a membranous slough, leaving the parts beneath covered with a layer of healthy granulation tissue. No movement of the bowels. Diet to be continued. Opiates to be given twice daily.

*29th December.*—The granulating surface is steadily diminishing in extent. There is no pain or uneasiness. Dressing continued as formerly. Diet the same. Opiate as before.

*19th January* 1870 (39th day from the descent of tumour, 35th from operation).—The wound is now almost absolutely closed, a small granulating surface about a quarter of an inch square alone remaining. To-day the bowels acted spontaneously. Dry lint dressing. Granulating surface touched with solid nitrate of silver.

*22d January.*—The patient has been out of bed upon the sofa for two days, and has, it seems, been moving about. The nurse says there were a few drops of fluid fæculence upon the dressing when removed this morning. There is, at the hour of visit, no appearance of any fæculent escape, nor any fæculent odour about the parts. The granulating surface is depressed, entire, and bloodless. The surface to be touched with nitrate of silver, and dressed with dry lint and a spica bandage. Patient to be confined to bed.

*31st January.*—The dressing has been undisturbed since last report. There has been no discharge of either pus or fæculence from the wound. The bowels have been moved naturally every day or second day since the 19th. The site of the operation is marked by a small depressed hollow; the surface is composed of sound cicatrix. There is no evidence of any tumour nor any bulging upon coughing. The part is supported by means of a pad and a spica bandage.

The features of chief interest in this case are:—

1. The period which elapsed between the descent of the rupture and the occurrence of symptoms of strangulation, amounting to four days.
2. The subsidence of these symptoms after lasting for a few hours.
3. The development of symptoms of suppuration within the sac four days after the symptoms of strangulation had subsided.
4. The sloughing of the peritoneal coat of the bowel without the involvement of the muscular or mucous coats.
5. The escape of the patient from the establishment of a regular fæcal fistula.
6. The long period which elapsed before the bowels acted spontaneously.
7. The spontaneous recedence of the knuckle of intestine within the abdominal cavity during the process of healing.

The development of well-marked symptoms of strangulation, after the lapse of four days from the period of descent, must, I think, be attributed to the efforts employed to effect the reduction of the hernia by means of the taxis, and must be referred to the development of inflammation of the sac, and, to some extent, of its contents.

When this inflammatory attack subsided, the symptoms of strangulation proportionately abated, although a sufficient degree of damage had been inflicted to determine the sloughing condition of the peritoneum lining the sac and covering the bowel, as well as that of the suppuration met with in the sac at the operation. To the injury inflicted by inordinate taxis may, I also believe, be attributed the sloughing of the serous coat of the bowel, while the mucous and muscular coats remained intact. Where the tightness of the constriction around the neck of the hernia gives rise to sloughing, we generally find it is the mucous coat which has been soonest cut through, by ulceration extending from within the gut at the site of constriction, imperilling the vitality of all the mucous membrane included within the line of ulceration. Here manifestly not only was there no such sloughing of the mucous membrane, but no such ulceration of the mucous coat. Had it occurred, on the separation of the slough a regular fæcal fistula must have become established. When the taxis has been very forcibly employed before resort has been had to operation, it is not uncommon to find a thin layer of blood extravasated beneath the serous coat, separating it more or less completely from the muscular coats beneath. Such separation must inevitably enfeeble the vital connexions of the serous coat; and when inflammatory action supervenes, it need be no wonder that extinction of its enfeebled vitality should ensue, and that separation of the whole serous coat, so far as involved in the form of a connected layer of membrane, should take place. So far as the symptoms, general or local, were concerned, there was no change in the condition of the patient indicating the propriety of operative interference upon the Wednesday, except that the swelling was slightly increased, and the skin did not glide so readily over the parts beneath. It was evident from this that the inflammatory change within was extending outwards, and that the textures were becoming agglutinated together, in anticipation of the pointing of an abscess.

In the division of the soft parts, the vascular condition of the parts, the infiltrated state and cohesion of the layers of tissue made it more and more apparent that on reaching the sac we should reach an abscess. The opening the sac gave vent to a quantity of putrescent pus, which had, however, no communication with the interior of the bowel. The putrescence, furthermore, had no fæculent odour. If the germ theory of putrescence and suppuration were deemed correct, it might be an interesting subject for speculation as to the manner in which the germs found their way into this hernial sac.

The escape of the patient from the formation of a fæcal fistula must be attributed to the sound condition of the mucous and muscular coats at the seat of constriction, together with the firm adhesions which fixed the bowel to the neck of the sac. In this manner the most tender part of the protrusion was supported during the period of sloughing, and after cicatrization. As to the escape of a few drops of fluid fæculence upon one occasion, I have doubts whether any such escape really occurred. Though said to have been observed by the nurse, it was seen by no one else, and it never recurred. The appearance of the discharge from the granulating surface, which had been touched two days before with nitrate of silver, was not unlikely to be mistaken for fæculence, so far as appearances went; and as the view of the discharge being fæculent was not corroborated by the smell being noticed to be fæculent in its character, I feel inclined to doubt if the fluid contents of the bowel did effect their escape. But if it had really been so, it could not be matter for wonder, as the patient had risen and walked about the ward for two days before, though cautioned against such a proceeding.

The long period of persistent absence of all alvine evacuation may, I think, be chiefly attributed to the nature of the patient's alimentation, to the use of opiates, and to the degree of interruption to free flow through the affected portion of intestine. I believe the safety of the patient was in no small degree attributable to this quiescence of the bowels, but more especially still to the resolute repression of every suggestion to employ laxatives or administer an enema. The absence of all alvine evacuation, and the withholding of all laxatives, would at one time have been considered a most injurious line of practice, when active purgation was deemed indispensable to relieve the distended intestine above the site of constriction, and to elude the risk of persistent distention of the enfeebled portion of bowel producing rupture of its softened tissues. A reference to any of the works of note upon hernia pertaining to the beginning of this century will abundantly illustrate this remark, and show what disastrous and fatal consequences resulted from such active treatment, as compared with the system of expectancy and repose nowadays so universally enforced.

The spontaneous recedence of the protruded and adherent knuckle of bowel within the abdomen, after the relief of the stricture, is not an unusual occurrence. More than one case of a like kind has been narrated by Sir A. Cooper, Mr A. Key, and by other and more modern writers, and should certainly encourage the adoption of this line of practice in cases of apparently sloughing intestine, firmly attached by adhesions to the neighbourhood of the neck of the sac. To have laid open the sloughing knuckle of bowel would undoubtedly have been the practice of some esteemed surgeons,—a method of procedure which, in this case, would undoubtedly have been followed by a very different result.

The following practical deductions appear worthy of enumeration :

1. Not to regard the use of the taxis in incarcerated hernial protrusion as devoid of risk, or preferable to operation.

2. Not to delay operative interference when local inflammatory symptoms continue and extend.

3. Not to regard the whole thickness of a knuckle of intestine as in a sloughing condition because the serous coat is so, and lies bathed in a fœtid abscess.

4. To prefer simple division of the stricture, where extensive firm adhesions exist, to breaking up the adhesions, and returning the bowel.

5. To abstain from all measures calculated to obtain evacuation of the bowels when a doubtful condition of the intestine is disclosed at the operation; to nourish the patient upon the food least likely to afford in digestion an abundant residuum; and to employ opiates, so as to keep all uneasy symptoms in abeyance.

## ARTICLE V.—*On the Structural Relation between Urea and Uric Acid, etc.* By Dr JOHN G. MACVICAR, Moffat, N.B.

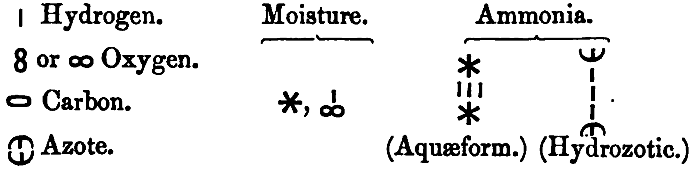
THE most approved chemical views throw but little light on the relation of these two substances, the latter of which often proves the cause of much suffering to man, while the daily genesis and excretion of the former in very considerable quantities is the index of health, and is necessary to its continuance. It is otherwise in other regions of the zoological scale. Different orders of animals, which, both in reference to their internal organization and their external movements, exist in striking contrast to each other (serpents and birds), agree in this respect—uric acid, not urea, is their normal excretion.

The chemical formulæ of these two substances, according as we adopt the older or the now more popular notation, stand thus:—

$$\begin{matrix} & \text{Old.} & \text{New.} & \\ \begin{matrix} C=6 \\ O=8 \end{matrix} & \left\{ \begin{matrix} \text{Urea, } C_2 H_4 N_2 O_2 \\ \text{Uric Acid, } C_{10} H_4 N_4 O_4 \text{ (bibasic)} \end{matrix} \right. & \left. \begin{matrix} C H_4 N_2 O \\ C_5 H_4 N_4 O_2 \end{matrix} \right\} & \begin{matrix} C=12 \\ O=16 \end{matrix} \end{matrix}$$

Here under neither notation does any homology or genetic relationship appear between them. But molecular morphology when applied to both shows a very beautiful and a very simple relation, which, if accepted by the physician, appears to me to be such as should prove of value to him. Moreover, this relation may to a certain extent be exhibited to the eye, and that by the use of such types as are already in the hands of the printer.

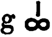
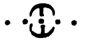
But there must be granted these two postulates:—(1.) That the law of symmetry, which is the acknowledged basis of mineralogy, botany, and zoology, is also the basis of the science of those material elements of which minerals, plants, and animals consist. (2.) That the following marks may stand for the organic elements occurring in the preceding formulæ. The first two are the atomic weights on the older scale of the substances they stand for. But they have also a symbolic value in reference to the supposed forms of the elements themselves. Thus, in the molecular morphology referred to,[1] hydrogen is an axial or bipyramidal, and oxygen an equatorial or double concave form; carbon is double convex; and azote is a coupled element, consisting of two menisci, resting on each other by their brims, each of which by itself is intensely active (but being dissymmetrical incapable of being insulated or obtained by itself), and is named zote.



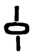
It is also further necessary to add, with regard to hydrogen, which is the simplest insulable structure possible under the law of symmetry, having only one material element, or unit of weight, for each pole or terminal of the geometrical axis, and three (which is the smallest number that can determine a plane) for its equator, that a symbol representing to the eye its full atomicity (that is, those regions on which it might possibly take into union with itself other atoms) would require to have two similar marks for the poles and three for the equator. Thus:—

Hydrogen · ⁚ · showing its possible atomicity.

Viewed in reference to tangent planes, it is a *trigonal* bipyramid (a doubly-pointed or shuttle trochar!); so also are sulphur, selenium, and tellurium. But all the other supposed forms of the elements, so far as they have been reached, are pentagonal on the equator. Thus, supposing the dot in the following symbol to stand for an atom of oxygen (as with the mineralogists), the equatorial atomicity of azote would be represented thus:—

Nitric acid (neglecting  = HO)  $= NO_6$

But to find the relation between urea and uric acid it is sufficient to attend to the case of hydrogen alone. And this we may do by following the synthesis of the simplest hydrocarbon under a progressive increase of carbon or escape of hydrogen. First, then, as a single element of carbon saturated with hydrogen, we obtain

Nascent marsh gas  $= HCH = CH_2$

But of such a structure the axis is too long for the extent of the equator; for symmetry culminates in sphericity. When, therefore, a couple of such structures meet, they double. And now they give an exquisite structure, in which all the five points for union in the atom of hydrogen which constitutes the axis are engaged, and the structure is finely differentiated and stable:—

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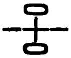
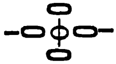
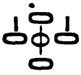
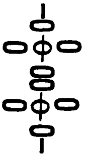
[1] See A Sketch of a Philosophy. Part II. Matter and Molecular Morphology. The Elemental Synthesis. Williams and Norgate.

Mature marsh gas  $= CH_4C = C_2H_4$

But the equator here consists of three naked arms of hydrogen. When the supply of carbon is greater therefore, it is to be expected that each of these will exist in union with an atom of carbon. Now, this union may take place in so many different ways, that we shall here have many different substances with the same chemical formula. That formula, taken in reference to a single member, must be $C_2H_4 + C_3$, that is. $C_5H_4$. But it may be shown that such members under the law of symmetry (sphericity) will tend to aggregate into tetratoms or groups of 4. The prevalent formula therefore will be $C_{20}H_{16}$. Here, then, we have the formula of non-oxygenated essential oils, and as the single element,—

Essential oil   & & $= C_5H_4$

Passing on now till the volatility of the hydrogen has enabled it to escape from this structure to the utmost, there remains only one atom of hydrogen as an axis of the structure, kept down by five atoms of carbon, one on each pole, and three on the equator. But an atom of hydrogen loaded with carbon to this extent cannot be raised by the chemist into the aeriform state or separated. It is therefore unknown otherwise than as a carbonaceous residuum containing a little hydrogen. When, however, a single atom more of hydrogen is attached, it may be sublimed in couples which are symmetrical, and in tetratoms which are spherical (overhead of their ethereal atmospheres). Thus the couple

Naphthaline  $= \frac{1}{2} (C_{20} H_8)$

But though the chemist cannot obtain the atom of hydrogen saturated with carbon $C_5H$ in a separate state, it plays, as might be expected, from its very perfect structure, a very important part in the economy of organic nature.

And here let us remark this wonderful range of functioning which belongs to an atom of hydrogen. While the substance in union on its poles may continue the same, the 3 substances upon its equator may range from 3H to 3C, with many lying between, as, for instance, n marsh gas, 3Cl, instead of 3H, giving chloroform, etc.

On the subject of oxygen, it is only necessary to remark here, that that element in its most active state exists in single atoms. But in its form, the single atom is so equatorial or oblate, that under the law of symmetry (sphericity), when free, these atoms go in couples, each couple forming an unit volume of oxygen gas, or volume equal to a single atom of hydrogen or of azote.

Both constituents of the atmosphere therefore are coupled elements.

Further, this also is to be remarked of both, that in the interior of both when coupled, in consequence of the concavity of the now central regions of the two parts of which each consists, there is a cell or cavity; and this is precisely a mould for an atom of carbon!

And when in this cavity an atom of carbon has succeeded in the course of vital action in lodging itself, it appears that the utmost analysis of which that action is capable cannot dislodge it again.

There thus remain to be eliminated from the animal frame the two homologous bodies—

= $CO_2$ = Dioxide of carbon—carboxygen.

— NC = Dizote of carbon—carbazote.

*Respiration.*—Of these, the former is isomorphous and isovoluminous with oxygen gas. Like this gas, it is also parasitic, and so it gives to nature a series of concrete carbonates, analagous to oxides. But it is now no longer mere oxygen gas, greedy of its appropriate food. Its stomach is now completely filled with carbon. In its relations to carbon, therefore, it is completely changed. It wants no more of it. And being symmetrical, and composed in so great part of oxygen, it tends to the aeriform state. And thus escaping from the blood by the lungs or the skin, it is expired, and gains the atmosphere, carrying along with it an amount of carbon, which, if left in the blood, would speedily prove fatal.

*Renal Action.*—But the latter, the dizote of carbon, cannot attain to symmetry and fitness for the aeriform state. For its azote is what remains of ammonia, in which it existed in union with three atoms of hydrogen; and this fact, added to the demand which carbon has for hydrogen, gives one atom of hydrogen at least always clinging to the atom of carbazote. Hence it remains as a hydracid. And hence, like other hydracids, it cannot in the course of nature be parted from moisture. It cannot be eliminated from the blood except in combination with water. And hence in the most perfect organizations, along with a respiratory, there is a renal apparatus.

*Urea.*—Separated or conceived as separate from water, this carbazohydric acid must be taken as a monohydrate—that is, as having attached to it an atom of HO on the non-hydrogerent pole—thus securing a certain amount of symmetry:[1]—

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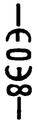
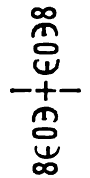
[1] The proper point of view is to regard these acid elements as fixed on the poles of an elemental drop of water, which is such that this (urea) matter will be to the water only 13 parts in 1000, though the law of symmetry shows that

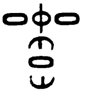
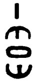
Nascent urea =  = $CH_2NO$

But under the law of symmetry (sphericity) this structure is obviously in the same predicament as our original hydro-carbon $CH_2$ (see p. 791). Its axis is too long for the breadth of its equator. Just as that hydro-carbon, therefore, tended to double into marsh gas, so will this tend to double into

Urea =  = $C_2 H_4 N_2 O_2$

Where and when in the course of the circulation this doubling takes place cannot now be inquired into. But if we suppose it to take place at the time when healthy urine from exhibiting an acid acquires a basic reaction, it would explain this change without our needing to go farther; for, as the hemi-urea is an acidous, so is urea a basic structure.

*Uric Acid.*—The construction of urea obviously requires that there shall be found at the commencement of the synthesis a simple atom of pure hydrogen only on the pole of the atom of carbazote. But in regions where loose atoms of carbon have been disengaged in abundance, and the action of the system is tending towards the construction of crystallogenetic and stable rather than of such easily transformable molecules as shall be answerable to the purposes of life, it is to be expected that 3 atoms of carbon will fix themselves on the 3 edges of the equator of such single atoms of hydrogen as are suitably placed for such union, and we shall have

Nascent uric acid =  instead of  + 3C;

and (not to overtax the printer with over-elaborate diagrams) after hydrating and doubling (and perceiving that the tetratom of hydrogen which forms the equatorial body in the atom of urea will, in this case, tend to grow into a symmetrically hydrated, that is, a bihydrated atom of ammonia), we obtain on each pole thereof the solidly constructed combination—

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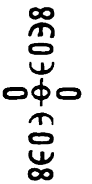
this quantity of urea may be doubled without danger to health or increased irritation. But this we must pass over at present, and confine our attention to the minim member.

Uric acid (monobasic),



$= C_5 H N_2 O_2$

The entire structure is

$$\overbrace{C_5 H N_2 O_2}^{\text{Pole.}}, \overbrace{\text{OHamHO}}^{\text{Body.}}, \overbrace{C_5 H N_2 O_2}^{\text{Pole.}} = C_{10} H_2 N_4 O_4 . 2HO. NH_3.$$

Thus do we obtain the only known urate of ammonia, which is also regenerated as often as uric acid and ammonia come in contact.

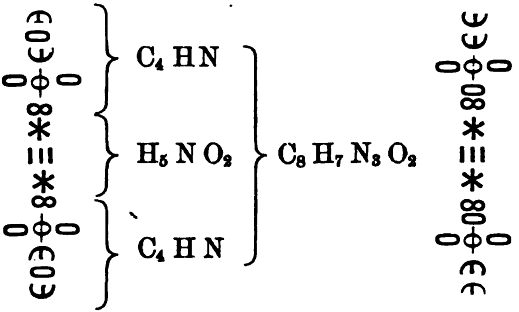
Besides urate of ammonia, uric acid itself, and urate of soda also, are plainly to be expected when the living system is bent on constructing uric acid, and, at the same time, is not capable of constructing urate of ammonia. But all except the last, whether generated in the blood or in the bladder, or between the two, are more or less abnormal in man, and must be prejudicial more or less to health.

The question then is, how may the easy construction of uric acid be specifically prevented; or, if formed, how may it be resolved into more soluble and less irritating substances? The theory (of Liebig) that it is the normal antecedent of urea, and undergoes, in the usual course of healthy vital action, transformation into urea, though not without beauty as a conception, is not supported either by clinical observation or the analogy of nature. And, indeed, the atom of uric acid when considered mechanically (or chemically, which is the same) is so stable, while that of urea is so tender, that though the transformation can no doubt be effected in the laboratory, yet in the human body, the problem is to prevent, as far as possible, the construction of uric acid in dangerous quantities.

One method of nature for this end appears to be to construct creatinine and hippuric acid, which are much more easily removed from the system, instead of mere uric acid. And another method (more useful but not so easy) appears to be to arrange, through the use of oxygen, the three atoms of carbon (which, by existing in an *equatorial* position, cause all the trouble) into an *axial* position, thus constructing the body of the lactic element, which is soluble enough, and which, when hydrochloric acid is in defect, can do the work of the latter, though not so well. But that by the way at present.

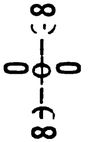
*Creatinine.*—Let an atom of the as yet uncompleted uric acid (see fig. p. 794) be fixed on an atom of symmetrically hydrated ammonia just as a completed atom of uric acid is in urate of ammonia, and an atom of creatinine results. This we may even attempt to show in a figurative formula (adopting a conventional arrangement of marks for OHamHO = $NH_5O_2$ = $HN_4O.HO$ = $NH_4.H.O.$, that is, bihydrated ammonia, a structure which plays a most important part in nature, but which the chemist has not yet succeeded in obtaining in a separate state):—

Creatinine



*Hippuric Acid.*—To construct this interesting body we must leave the region of single members of molecules, to which alone our attention has hitherto been directed, and come to the molecules resulting when these generally very lengthened members place themselves as radii around a common centre, and thus fulfil the law of symmetry (sphericity.) Happily for the progress of science only the numbers of 20 and 12, corresponding to the trigonal and pentagonal regions of union which were alluded to at the commencement of this paper, occur in nature.

Hippuric acid is benzoic acid, its poles surmounted by a nascent uric form, most probably arranged thus—

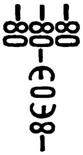


$= C_3H_3N\,O_2$;

and benzoic acid is a finely balanced dodecatomic molecule, its true formula being (as most usually) double that given in chemical works. For chemical formulæ having generally been estimated in reference to some substance whose atomic weight is known on *one pole* instead of on *both poles*, as the law of symmetry requires, give commonly half molecules.

*Glycocoll.*—The polar matter which differentiates the atom of hippuric acid, creatinine, and its hydrated state creatine, as also many other molecules in the animal organism, is always the same, namely, the tricarbide of hydrogen in union with the same carb-azotic or carbo-zotic matter, this latter being dimorphous, as I have endeavoured to show in the above two forms of creatinine. But viewed as existing in a separate state, this structure is not in keeping with the molecular structures of the terraqueous globe generally. It is dry, and it contains no oxygen. It must soon be moistened by aqueous matter, and that in the form of HO, a form which supplies oxygen and hydrogen at the same time. Now there are on it four regions (namely, the three naked atoms of carbon surrounding the atom of hydrogen and the azotic pole), on each of which an atom of HO may fix, and on which, if such atoms do fix, all will be duly moistened. We shall thus obtain the polymorphous structure of

Nascent glycocoll



$= C_4\,H_5\,N\,O_4$

The chemical formula, as is usual with neutral substances, is cut as close as possible. But to explain the physical and physiological characters of glycocoll, and to place it in its right position among the sugars, we must regard it as a differentiated dodecatom as is usual with us, which, in this case, requires us to double the chemical formula twice. When this is done we obtain, as the remarkable result, an atom of sugar with one of urea on each pole!

$$\text{Sugar of Gelatine}\left\{\begin{array}{lllll}\text{Urea,} & C_2 & H_4 & N_2 & O_2\\ \text{Sugar,} & C_{12} & H_{12} & & O_{12}\\ \text{Urea,} & C_2 & H_4 & N_2 & O_2\end{array}\right\} = 4(C_4\,H_5\,N\,O_4)$$

$$\overline{C_{16}\,H_{20}\,N_4\,O_{16}}$$

Here, therefore, the unorganized matter of the undigested food, of the bile, and of the broken-down tissue, instead of charging the system with uric acid, will give urea and sugar, which will increase and decrease *pari passu.*

It is not to be inferred, however, that in this metastasis from the uric, we have the true theory of the diabetic diathesis. Possibly where the vital functions are being vigorously performed, the sugar generated in this way may be given to the blood so early that it may serve the same purpose in the economy as the sugar generated in the liver. And what is that? To this it is to be answered that molecular morphology shows that both terminals of those members which, when symmetrically aggregated around a common centre, constitute an albumenoid molecule, are the simplest and sweetest of all organic structures, the very A B C of vitalized nature, namely, CHO, the simplest hydrate of carbon, which I have called the *saccharine*, because the proper molecule belonging to it, is $(CHO)_{12} = C_{12}\,H_{12}\,O_{12} =$ an atom of sugar. Where a molecule of an albumenoid, therefore, is going to be constructed, nothing can be of greater value to give it a start than an atom of sugar. And probably there are few functions in the body which are of greater value than the glycogenic power of the liver. Possibly, therefore, this sugar, like the lactic acid, by the genesis of which also the construction of uric acid is prevented, may in good constitutions be turned to a good use.

But these views also give a theory of the true diabetic diathesis. Thus, let the system be in such a state that, instead of being competent to construct fresh albumenoids when saccharine atoms are given as a nucleus, it lets those which already exist soak or rot from the periphery to the centre, in overmuch water, and then by every albumenoid molecule thus lost, an atom of sugar will at last be exposed; and a deplorable state of things must result,—a state in which the albumenoids of the blood, instead of normally opening up, engaging moisture, and joining hand in hand to constitute tissue and impart beauty and mellowness and muscularity, are being digested into mere urine, of which there is thus far too much for health, both as to quantity and weight. Such a state of things it is anything but cheering to contemplate, either as to its issue or its cure. But when the physician sees his way, there is no saying what he may be able to do.

## ARTICLE VI.—*On Croup.* By WILLIAM KERR, Surgeon, Braehead, Galt, Canada West; Corresponding Member of the Medical and Physical Society of Calcutta, and of the Medico-Chirurgical Society of Glasgow.

CROUP, as is well known, soon passes the stage in which remedies are of any avail, and medical men are forced to be mere spectators of agony which they cannot relieve, and which they and the parents are constrained in kindness to long to see terminated by death. To one of such hopeless cases I was called in December 1859, on the fourth day of the disease. The breathing was distressingly difficult and croupy; the skin near the lower insertion of the sterno-cleido-mastoid muscles was sucked down during inspiration; cough croupy; tongue white; pulse frequent; lips and tips of the cheeks livid; other parts of the face chalky white. Emetics and calomel had been given, and the time for bloodletting had passed away. Possibly, some might have advised tracheotomy; but as this at best is a doubtful remedy, condemned by our most judicious writers, I resolved to try the digitalis combination without opium.[1] A few hours were lost in ascertaining the necessary dose, but fifteen minutes after two and a half grains, the age being four years, she was asleep, and in other fifteen the croupy breathing suddenly ceased, without the child having awaked or expectorated. On awaking, two hours afterwards, the breathing became more noisy, but another dose procured sleep, and restored easy breathing. Next day she was better in every respect, and in a few days was quite well. The greatest quantity given in one day was seven and a half grains, and the total quantity thirty-seven and a half, or fifteen doses.

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[1] For details respecting the composition of this medicine, the reader is referred to the numbers of this Journal for October 1869 and June 1865. I may say here, that it consists of two members of the Solanaceæ, Dulcamara and Stramonium; and three of the Umbelliferæ, Sium lineare, Cicuta maculata, Cœnio-selinum Canadense, and either Digitalis, Squill, or Bisulphite of Soda; these three varieties being required in diseases of the mucous membrane to suit differences of age, of individual, and of epidemic constitution.

In the next instance, I was called, on the second day of the disease, 6th January 1860, to a child of fourteen months, to whom an emetic and purgative had been given, without, however, procuring relief to breathing. The respiration and cough were croupy, but the colour of the countenance and its expression were natural; pulse 156. He was a robust-looking child, yet the veins of the hands and feet were thready in diameter. I opened one on the wrist, which, for its size, bled freely; and by immersion in warm water, though bleeding was prolonged for two hours, the total quantity of blood lost was trifling, and did not produce faintness, or the marked remission in the symptoms which bloodletting usually causes. A powder containing nearly two grains of the digitalis combination was given, but it was not till the third was administered, six hours afterwards, that a marked effect was observed. It had not been swallowed many minutes when he fell asleep, and his breathing soon after became easy—indeed, so suddenly, as to warrant the belief that the difficulty of breathing had not proceeded from the accumulation of mucus, or the formation of a false membrane; there was no expectoration to account for the change. In the next sixteen hours six powders were given, and between nine and ten hours of sleep obtained. The breathing, though still croupy, was much improved; the skin over the spaces near the lower insertion of the sterno-cleido-mastoid muscles was not sucked down in inspiration, as at the commencement of the treatment, and for a short time he played with the other children. This, however, was his best period, and for six hours he did not require a dose, but difficulty of breathing with cough returned; and though in the following twenty-two hours eleven doses, several of them the squill combination, were given, it is stated in my notes that the general character of this period was a short amount of sleep, owing to difficulty of breathing and frequent cough. I had now given thirty-six grains, of which the twenty last administered had not prevented the disease from gaining ground. The bowels had been kept sufficiently open, and the child had been put into a warm bath without benefit. Disheartened by want of success, I abandoned all treatment, and he died twenty-seven hours afterwards.

A subsequent case has possibly thrown light upon the cause of failure. A full-bodied child of sixteen months, twenty-four hours ill, though treated with ipecacuan, calomel, and tartrate of antimony, was getting worse. In consultation with Dr Pipe of Berlin in this neighbourhood, two grains and a half of the squill combination were given; in fifteen minutes sound sleep ensued, and croupy breathing ceased. Returns of croupy breathing required three doses in the next thirteen hours, each dose being followed by sleep and easy breathing. Another return, somewhat worse in its character, indicated that the disease, though checked, was not subdued. The child's fatness obscured every vein, except the external jugular, which was opened, and a considerable quantity of blood abstracted in a full stream, without, however, producing faintness. When the flow of blood was stopped, the breathing had become easy, and a few doses of the squill combination completed the cure. In my notes of the fatal case preceding this, it is remarked that the child's countenance remained ruddy, and his eyes clear, till twenty hours before death. These circumstances, together with the result of the case just related, led me to think that I ought to have proceeded to open the external jugular, undeterred by the apparent want of blood in the veins of the wrist and feet.

I have met with no cases, besides the two last, so inflammatory as to require bloodletting; the others readily yielded to the squill or digitalis combination, premised in some by an emetic.

Dr Mackintosh of Hamilton reports four cases of croup in 1864. They were seen in an early stage, and were cured by either the digitalis or squill combination alone; the relief in all was most marked, and almost immediate. In the winter of 1865, a stout boy, aged three years, was seized with croup; there was high fever, flushed face, and rapid pulse. Two and a half grains of the digitalis combination were given every three hours; relief was speedy and permanent, and in four days he was quite well. A child of twenty months had an attack more urgent than the preceding, but by the same treatment was cured in a few days. Dr Mackintosh, in a letter dated 30th July 1869, says, "I have not kept notes of cases for the last four years, but I always use either the digitalis or squill combination in croup. I am not aware that they have ever failed."

Dr MacIntyre of Hespeler was called to a boy, seven years of age, who had been ill for twenty-four hours, during the last four of which respiration was most difficult, the skin near the lower insertion of the sterno-cleido-mastoid muscles being sucked down at each inspiration; the lips and tips of the cheeks livid; the other parts of the face chalky white. An emetic gave considerable relief, but breathing being still difficult and distressing, a dose of the digitalis combination was given; in fifteen minutes he was asleep, and soon after croupy breathing ceased. Four doses were administered daily, and in a day or two he was quite well.

In the several cases of croup, cessation of dyspnœa was unaccompanied by expectoration. I was much surprised when I first witnessed a child with loud croupy breathing, and every symptom of air entering the lungs with extreme difficulty, breathe easily, and without noise, in the course of half an hour, no mucus having been brought up to account for the change, and only a trifling amount coughed up during recovery; but I have now been long familiar with the fact. Relief given by bloodletting, when this remedy is successful, is equally rapid and great, and cannot be accounted for by expectoration, which, unless the child vomits, seldom occurs. It is therefore chiefly to the abatement of the inflammatory condition of the lining membrane of the trachea and bronchiæ that the benefit derived from both remedies is to be attributed. These facts confirm the opinion of Dr Cheyne and others, that dyspnœa in croup does not arise so much from the exclusion of air by the secretion of mucus, or the formation of a false membrane, as from the inflamed state of the respiratory passages.

Most cases of croup, and possibly all in the advanced stage, having not only the larynx and trachea, but also the bronchiæ inflamed, and containing a considerable quantity of viscid mucus or mucopurulent secretion, bronchitis in such cases forms a portion of the disease, and the conclusion is inevitable that the same remedy is adapted to both. Several of my correspondents report to me generally their success in bronchitis unmixed with croup, a success which corresponds with my own experience. The dyspnœa diminished, the livid countenance regained its natural colour, the cough became less frequent, indicating that the copious mucus or mucopurulent secretion was lessened, and likewise more easily expectorated. When cough is incessant and severe, a tincture of the digitalis or squill combination is preferable to any other form, owing to the whole dose entering the system at once, thus overpowering the cough, which speedily ceases, and the patient falls asleep. The returns of coughing become less frequent, and, possibly in consequence of longer intervals of rest, and the direct influence of the medicine in healing the tender mucous membrane, recovery goes on, and phthisis, I am satisfied, in some instances averted. These remarks are intended merely as suggestions for farther trials in countries where bronchitis is more common than in Canada. The difference in frequency between Scotland and Canada cannot be better illustrated than by saying that here a cough is scarcely ever heard in winter during divine service in a place of worship.

I have now endeavoured to show that the same, or nearly the same remedy, with its varieties, is adapted to dysentery, summer cholera, scarlet fever, croup, and bronchitis—diseases widely different in their symptoms, yet possessing a common bond of alliance in having their seat in the mucous membrane. Is it irrational to believe that other acute diseases, having likewise their seat in the same membrane, may be curable, and easily curable too, by the same remedy? I could give additional grounds for this belief by cases of several of these diseases which have occurred to my medical correspondents or myself, but I do not think that they are as yet sufficiently numerous to carry conviction into the minds of others. In conclusion, I may say that I have never found the combinations I have recommended of any utility, except in diseases of the mucous membrane.

In the second volume of the Transactions of the Provincial Medical and Surgical Association of England, 1834, Mr Grindrod relates a case of hydrophobia, and illustrates it by a coloured plate, showing great vascularity of the mucous membrane of the trachea, pharynx, œsophagus, stomach, and duodenum, accompanied with incipient ulcerations in the lower part of the œsophagus. In a disease hitherto without a remedy, may I suggest trials of the preceding combinations?

Dr Clarke of Paris, in this neighbourhood, in a case of severe confluent smallpox, occurring in a vaccinated person, reports that the condition of the throat and upper portion of the respiratory passages, and of the function of respiration, rapidly improved when he commenced the use of the digitalis combination, which for several successive days was the only medicine given. This case was likewise seen by Dr Dickson of Paris, who was equally satisfied of the benefit derived.

During the hottest weather of a Canadian summer, an affection of the lips and gums occasionally prevails, in which these are swollen and painful, with numerous small ulcers upon their surface. Dyspepsia is frequently an accompaniment. Left to itself, the complaint may continue for weeks, producing considerable distress, but without danger. In every instance which has occurred to me, relief and a speedy cure followed the administration of the digitalis or squill combination. I have not had an opportunity of trying that with bisulphite of soda. I mention this affection because its decline under medicine is as patent to the eye as an experiment in a laboratory, and because I am not aware that it has been noticed by any author.

## ARTICLE VII.—*Remarkable Case of Fibro-nucleated or Cancroid Growth in the Pericardium.* By R. HOGARTH CLAY, M.D. Edin., Physician to the South Devon and East Cornwall Hospital, Plymouth.

RUTH WOOD, aged 37, widow, no children, was admitted into the South Devon and East Cornwall Hospital on 1st December 1869, under my care.

She had been a worker in the Government Dockyard at Devonport, and had enjoyed good health till about two years ago, when she was seized whilst at work with pain over the region of the heart, followed by syncope. Since then she has had occasional palpitations. Six weeks before admission she "caught a violent cold," with pains in her limbs and general prostration. She was kept in bed until the day of her admission. Father, mother, and two brothers are living. One brother is in delicate health. Two sisters have died during the past two years, with symptoms, her r states, such as the patient now presents. Since her first attack she has been temperate, though before that time she was occasionally much given to intemperance. No history of syphilis.

When admitted, her appearance was that of a person suffering from some disease of the central organs of circulation; her face swollen and livid; lips blue; extremities cold; short, dry, spasmodic cough, with most urgent dyspnœa. Pulse 120. Temperature about 98. Respiration 40. Pain in the epigastrium; unable to lie down; on both sides of her neck, just above the clavicles, were groups of enlarged, hardened, and painful glands; the left arm and breast were slightly swollen; over the upper part of left front were numerous enlarged capillaries, and one or two enlarged veins.

There was absolute dulness on percussion over the entire left side in front, most pronounced over the upper third, with a remarkable feeling of resistance; the dulness extending about one inch to the right of sternum. There was no vocal resonance, no vocal fremitus, no tubular breathing, but just below the external end of the clavicle breath-sounds were now and then faintly audible. There was general fulness of the whole side and of the intercostal spaces; no expansion or inspiration. Over the entire same side behind there was absolute dulness on percussion, with increased vocal fremitus, tubular breathing, bronchophony, and very slight fine crepitation.

The heart-sounds were normal, though feeble and distant; the apex beat was barely to be felt; the heart was not misplaced; the radials were the same at both wrists; the pupils equal in size. The expectoration, which was of clear mucus, was very scanty and difficult. All the other organs were, as far as could be determined, normal. The urine contained chlorides from the first.

Under the use of diuretics and diaphoretics, with nourishment and stimulants, her urgent symptoms were somewhat relieved. She was enabled to lie down, and always lay on the left side. On measurement of the two sides of the chest, the left was generally found to be one inch larger than the right, on which side there was no evidence of anything amiss.

She remained in this state, with very little variation, till her death. The dyspnœa and cough now more now less urgent; the physical signs nearly the same; now and then no breath-sounds could be heard under the left clavicle, and again at times they became, as at first, faintly audible. The pulse varied from 120 to 140; respirations from 40 to 50; temperature generally about 98°, though, two days before her death, going as high as 100° to 101°, and dropping again to 98° on the day on which she died. The treatment consisted of small doses of pot. iod. and ammonia occasionally, and diuretics and aperients, and doses of sp. æth. sulph. co. to relieve the dyspnœa. There was no hope entertained of her recovery, and medicines were only given to endeavour to relieve suffering.

*Sectio cadaveris, six hours after death.*—Body not much emaciated; still warm.

On opening the thorax, it was found that the left pleural cavity contained about two pints of clear serum; there were also numerous old and very strong adhesions, chiefly in the anterior and lateral regions; those near the base were very long, some being about four inches in length, and cord-like. There were a few adhesions on the right side, but no fluid.

The pericardium appeared like a large solid growth, filling up the whole of the upper two-thirds of the left side, extending one inch to right of middle line, and as far down as the ensiform cartilage. On opening it, there escaped about ten or twelve ounces of bloody serum. It was greatly thickened, the thickness being about two inches in the upper part, and gradually decreasing downwards, till, over the apex of the heart, it did not exceed one-eighth of an inch. It was of soft sarcomatous density and colour, not brain-like or encephalomatous, nor did it yield on pressure a milky juice. The pericardial cavity was distended, its serous layer perfectly smooth, and quite healthy. There were no adhesions and no white patches. The morbid structure appeared to be entirely limited to the external or fibrous layer. The heart was soft and flabby-looking, as if it had been soaked in fluid for a long time. The fat on its surface was nearly all absorbed, what remained being puckered like the skin of a finger after prolonged poulticing. The valves were healthy. There was no hypertrophy. The left lung was compressed and carnified, entirely non-crepitant, and reduced in size to about five inches in length and four inches in breadth. It did not contain tubercle nor any other morbid material. The right lung was gorged with blood, but was everywhere crepitant. There were no cicatrices at the apex, nor did it contain tubercle. The liver and kidneys were slightly congested; all other organs quite healthy. The glands in the neck and mediastinum were filled with black deposit, and enlarged. There was a little milk in the left mamma.

*Microscopical Examination.*—The diseased structure consisted of fibrous tissue throughout, which was infiltrated with a multitude of naked nuclei, of globular form, about the 2500th of an inch in diameter, the edges of which became thicker and more distinct on the addition of acetic acid. The enlarged bronchial glands were hypertrophied, the gland elements multiplied and loaded with black pigment.

I am indebted to Dr Hughes Bennett of Edinburgh for the microscopic appearances. He in the kindest way took the trouble to examine the structure, and gave me his valuable opinion as to its nature.

# ARTICLE VIII.—*On the Action of the Cobra Poison.* By J. FAYRER, M.D., F.R.S.E., C.S.I.; Surgeon, Bengal Army; Professor of Surgery in the Medical College of Bengal.

(*Continued from page* 624.)

## TWELFTH SERIES.

SINCE my last report on the subject of snake-poison, I have received a communication from Professor Halford of Melbourne, whose researches have already thrown so much light on this interesting pathological question. In a paper, of which he has kindly sent me a copy, read before the Medical Society of Victoria, he strongly advocates the injection of ammonia into the circulation; he also details several interesting experiments, as well as cases of snake-bite, in which the results were satisfactory.

This mode of treating poisoning not only by snake-bites but by chloroform, hydrocyanic acid, and other toxic agents, among which pyæmia is mentioned, and cholera suggested, has evidently been received with much confidence in Australia, and the matter is fully and ably discussed in the paper in question. The subject also of the structural changes in the blood, to which I have frequently adverted in former papers, and which were described by Professor Halford in 1867—vide *British Medical Journal*, 20th July and 21st December 1867[1]—is also referred to with some further expla-

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[1] "When a person is mortally bitten by the cobra-di-copella, molecules of living 'germinal' matter are thrown into the blood, and speedily grow into cells, and as rapidly multiply, so that in a few hours millions upon millions are produced, at the expense, as far as I can at present see, of the oxygen absorbed into the blood during inspiration; hence the gradual decrease and ultimate extinction of combustion and chemical change in every other part of the body, followed by coldness, sleepiness, insensibility, slow breathing, and death.

"The cells which thus render in so short a time the blood unfit to support life, are circular, with a diameter on the average of one seventeen-hundreth of an inch. They contain a nearly round nucleus of one two-thousand-eight-hundredth of an inch in breadth, which, when further magnified, is seen to contain other still more minute spherules of living 'germinal' matter. In addition to this, the application of magenta reveals a minute coloured spot at some part of the circumference of the cell. This, besides its size, distinguishes it from the white pus, or lymph-corpuscle.

"Thus, then, it would seem that, as the vegetable cell requires for its growth inorganic food and the liberation of oxygen, so the animal cell requires for its growth organic food and the absorption of oxygen. Its food is present in the blood, and it meets the oxygen in the lungs; thus the whole blood becomes disorganized, and nothing is found after death but dark fluid blood, the fluidity indicating its loss of fibrine, the dark colour its want of oxygen, which it readily absorbs on exposure after death.

"Let it not be thought that microscopic particles are unable to produce such great and rapid changes. It is well known, and I have frequently timed it with my class, that a teaspoonful of human saliva will, when shaken with a like quantity of decoction of starch, convert the whole of the latter into sugar in a little less than one minute. If ptyaline, the active principle of saliva, exerts this power at most in a few minutes, then surely the active principle of the secretion of the serpent's poison-gland may exert an infinitely greater power in as many hours. It results, then, that a person dies slowly asphyxiated by deprivation of oxygen, in whatever other way the poison may also act; and so far as the ordinary examination of the blood goes, the post-mortem appearances are similar to those seen after drowning and suffocation.

"I have many reasons for believing that the *materies morbi* of cholera is a nearly allied animal poison. If so, may we not hope to know something definite of the poisons of hydrophobia, smallpox, scarlet fever, and indeed of all zymotic diseases?"

"The following was the result of numerous experiments on dogs and cats. Blood soon nations, and which, so far as I can understand it, modifies the views as at first expressed by Dr Halford. In the paper to which I have referred, Dr Halford says of those corpuscles, "he had never seen those cells *before death*, but he believed the organic germinal matter of the serpent's poison to be the efficient agent, and the post-mortem changes in the blood to be in some way connected with a metamor-

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drawn from an animal bitten by a snake contains a larger amount of nebulous or finely-granular matter than is usually seen. After the lapse of one hour this nebulous matter is much increased in quantity, lying in the intervals of the red corpuscles, and presently it breaks up into small masses, out of which the cell is gradually evolved. In two hours after the bite the cells may be seen in great numbers, but very indistinct. From this time every further microscopic observation shows them in great abundance; and from the sixth to the twelfth hour they may be seen in perfection, macula and nucleus included. Whilst this is taking place the nebulous matter disappears; the nebulous matter must, therefore, be regarded as the germinal matter out of which the cells are formed. At this time the cell-wall is extremely delicate, the macula very plain as a bright particle, and the nucleus either single, reniform, double, triple, or multiple.

"It would appear that the cells are now increasing in number by division of their nuclei, and the minute particles, having the vibratory movement of molecules in fluid, may be seen between the nucleus and cell-wall. On one occasion we watched for upwards of half an hour a constant revolution within the cell of a particle corresponding in all particulars to a macula. This particle passed regularly round the nucleus at a uniform rate, revolving both in the direction of and against the current of the fluid in which the cell was flowing, reminding one of the movements seen in valisneria, etc. Twenty-four hours after the bite, the cells attain their greatest size, and, supposing the animal then dead, have probably ceased multiplying, and are simply living or perhaps growing, the nucleus being usually single, the macula extremely distinct, and the cell very large. It is not uncommon at this time and later to see a cup-shaped hiatus in the cell-wall from which the macula has escaped. The cells may be seen in the blood for many days, their presence seeming to be preservative against putrefaction. Where they have most room, as in the venæ cavæ, cranial sinuses, and cavities of the heart, they attain the greatest size and most circular form. In every instance the cell-wall is very elastic, and accommodates itself to surrounding pressure.

"To ascertain how soon after inoculation these cells appear, is a matter of some difficulty. It is not necessary to suppose that at first they are very numerous; and, in order to detect them so early, it might require fifty or a hundred microscopes and observers at work at the same instant. Still, from their having been seen two hours after the bite, and from all we know of the rapidity with which new formations occur, both in health and disease, it is doubtless extremely soon. Of one thing we are sure, viz., that the nebulous germinal matter from which they spring is within a few minutes diffused all over the body; for supposing an animal to die in five minutes, and hence all circulation stopped, the cells are as readily seen in its blood a few hours after death as if it had lived as many hours as we say minutes. The macula is, doubtless, a particle of germinal matter; but, whether it is to be regarded as that from which the whole cell has sprung, or whether it has been detached from the nucleus and is destined for independent existence, it is difficult to say. The fact that it is almost invariably large when the cell is small, and small when the cell is large, favours the first view. Perhaps the most important point must be left still undecided. Has the blood built up these cells, directly or indirectly, from the germinal matter of the serpent? The answer to this question the professor would endeavour to give at a future meeting; but in either case the result was the same, storing up of force in the new growth, at the expense of the nutritive properties of the blood, and by perversion of those chemical changes necessary to the maintenance of the life of the infected animal.

"That the germinal matter exists in a state of extreme minuteness, the following experiment shows:—A cat, being with young, was inoculated with the poison, and, dying in three hours, her four kittens were removed from the womb. They were dead, and the blood of all contained the foreign cells, as did that of the mother. To pass from the cat to the kittens, the germinal matter must have penetrated the delicate membrane covering the tufts of the fœtal vessels. If the poison of serpents can thus readily be traced through the body, and from parent to offspring, why should not the path of all infections be tracked? Some months ago it was stated that it was conjectured that a child had been bitten by a snake. No doubt need ever exist for the future; a drop of blood will always furnish the necessary evidence. He trusted the subject would not be let fall to the ground in Victoria, for it would assuredly be taken up at home. It had been to him a matter of surprise that, while this colony very properly appoints men to survey her coasts, explore her skies, and the ground beneath her feet, no one systematically explores her diseases—a subject in which the rich and poor, the living and those about to live, are equally and deeply concerned, and in comparison with which many other subjects that excite her people are trifles."

phosis of the fibrine of that fluid, which, so far as coagulation was concerned, appeared destroyed by snake-poison. It was also the case in death from hydrocyanic acid." I had certainly understood from Dr Halford's former writings that these corpuscles were an ante-mortem condition, a development of cells in the living blood "at the expense of the oxygen absorbed into the blood during inspiration, and hence the gradual decrease and ultimate extinction of combustion and chemical change in every other part of the body, followed by coldness, sleepiness, insensibility, slow breathing, and death." If I do not misunderstand him, in one paper Dr Halford describes the formation of the cells as an ante-mortem change, and the actual cause of death; but in the later paper, I read that these cells are never seen before death. My examinations have been confined to the poisoned blood, during the life of the animal or immediately after its death, and I confess I have failed altogether to find them; as post-mortem changes, they are no doubt very important and interesting, and I shall certainly search for them, but as such, I think, they can hardly be regarded as the cause of death.

My impression is still very strong that death from snake-bite, when it takes place within a short period, as it always does in an animal thoroughly bitten by a cobra, is due, not to any organic changes in the germinal matter or cellular structures of the blood, for which, indeed, there is often not time. But as life may be suddenly destroyed by such poisons as hydrocyanic acid before any blood change can possibly occur, so in the case of the bite of a vigorous cobra in a small animal, death occurs almost, if not quite as instantaneously, but by its direct influence on the centres of nerve force, by exerting an antagonistic force—one that is incompatible, in short, with those which regulate and govern the phenomena of life. I have already expressed an opinion, which I repeat, that when death occurs more slowly, and when time is given for blood changes to take place, that such do probably occur as in other *toxi toxæmiæ*, and that the man or animal dies therefrom in a similar manner to that in which he or it might have perished from any other form of blood-poisoning.

Present—Dr Fayrer; Dr J. Ewart, Professor of Physiology; Mr Sceva.

EXPERIMENT No. 1.—*29th May* 1869.—At 2.48 P.M. the femoral vein of a middling-sized healthy dog was carefully exposed, and ʒi. of liquor ammoniæ, sp. gr. ·959 B. P., was injected into it with the hypodermic syringe. The dog lay still for a moment, and was then raised; he howled loudly, and fell on his belly, the legs being unable to support his weight. Lay in that position in a general state of tremor, involuntary defecation and micturition taking place. 2.50.—Lying on his side; convulsed and twitching in every muscle of the body; pupils widely dilated. 2.52.—Lies on his side in a state of unusual muscular twitching; unable to rise, or to walk when raised. 2.55.—Starts up and tries to run; falls down; is unable to rise again; the head fallen only on one side. 2.57.—Lying quiet, cannot stand when raised; no tremor now; breathing hurried and deep. 3.4.—Seems to be recovering; raises his head, and tries to get up; breathing still hurried and deep. 3.10.—Recovering; raises his head, makes efforts to get up; profuse salivation and frothing at the mouth. 3.13.—Sitting up; licking the puncture; refuses water when offered; great salivation. 3.30.—Is apparently well again; walks about as if nothing had occurred. The object of this experiment was to test the effect of the liquor ammoniæ injected into the venous circulation in an animal uninfluenced by the poison. It was used of the sp. gr. ·959 B. P., as directed by Professor Halford, and it was injected into the femoral vein in the manner suggested by him. The impression produced by this experiment was that the dog had a very narrow escape from death, and that the effects of the ammonia had nearly proved rapidly fatal.

EXPERIMENT No. 2.—A large and powerful dog had the right femoral vein exposed, and was then bitten by a fresh and full-grown spectacled cobra (*gokurrah* of the snake-men), in the integumentary fold of the left thigh, at 3.6 P.M. 3.8.—Began to show signs of the effect of the poison; staggers; is slightly convulsed; micturated. Forty minims of the liquor ammoniæ, sp. gr. ·959 B. P., were now carefully injected into the femoral vein already exposed, with the hypodermic syringe. 3.9.—Violently convulsed; but raising the head and trying to rise. There could be no doubt that whatever the later effects might be, the immediate consequence of the ammonia injection was to make the animal much worse. 3.10.—Stood up; breathing very rapidly; salivation profuse. 3.12.—Breathing hurried; sitting up and looking more intelligent. 3.13.—Is able to stand alone. 3.15.—Lies down; salivation very profuse. 3.20.—Is certainly better; walks, but drags the injected leg; is sluggish. Remained in this condition, very restless; lying down and rising; drowsy at 3.30; thirty drops more of the ammonia injected. 3.31.—Lying down; is drowsy. 3.40.—Is lying down; being sluggish, with hurried breathing. 3.43.—Worse; hypodermic injection of the ammonia; forty drops under integument of fore-leg. 3.44.—No apparent effect; twenty drops more injected in the same place. 3.50.—Is worse; convulsed. 3.53.—Involuntary defecation; breathing catching, and rather slow; seems quite exhausted; pupils widely dilated; lips pallid. 3.54.—Dead. Bitten at 3.6 P.M., dead at 3.54, in forty-eight minutes. The results of this experiment are not favourable to the ammonia theory. Death took place in about the usual time in which it occurs in a dog after a bite from a vigorous cobra. The effect of the first injection impressed one with the idea that for a time the influence of the snake-poison was in abeyance, but the later symptoms were rather unfavourable than favourable to the ammonia.

Experiment No. 3.—A fowl was bitten by a cobra in the wing, at 3.36 p.m. Half a minute later, I injected twenty minims of the liquor ammoniæ into the femoral vein, which had been previously exposed. 3.38.—Violently convulsed; the convulsions passing rapidly into a state of general tremor and death. The fowl was bitten in the wing, where the parts were not very vascular, that the poison might not be absorbed so rapidly as if bitten in the fleshy part. The poison had no time to manifest its effects, for the injection of the liquor ammoniæ was followed by immediate convulsions and death.

Experiment No. 4.—A fowl had ten minims of the same liquor ammoniæ diluted with twenty minims of water, injected with the hypodermic syringe under the skin of the thigh. 3.44.—Apparently not affected. 9 p.m.—No change. *30th*, 6 a.m.—No change. *31st*, 8 a.m.—The fowl keeps the leg drawn up, but is not otherwise affected. Apparently, beyond slight local inflammations, no effect produced.

Experiment No. 5.—The dog of experiment No. 1 having perfectly recovered, had the left external jugular vein exposed, at 3.55 p.m. of the 29th May. He was then bitten in the right foreleg by a fresh full-grown spectacled cobra. 3.56.—Sits down, rises and walks about, limping on the bitten leg. 3.58.—Sits down, but roused, walks about. 4 p.m.—Sluggish, lies down, walks unwillingly, ears drooping. 4.2 p.m.—Rises and walks about with uncertain gait. 4.3 p.m.—Lies down, with the head on the ground, apparently in a partially exhausted state. 4.6.—Stands, but is unsteady, head hanging down, and with salivation. 4.11.—Staggers in his walk. The dog had only one bite, and the poison is now evidently taking effect; so forty minims of the liquor ammoniæ were carefully injected with the hypodermic syringe into the jugular vein, the greatest care being taken not to admit any air with the fluid. The dog was immediately convulsed violently, fell over, was quite unable to stand; the convulsion passed into rapid jactitations of all the muscles. 4.15.—Perfect muscular exhaustion, hurried breathing. 4.18.—Injected twenty minims more of the liquor ammoniæ into the vein. Convulsive movements again became universal, pupils dilated, involuntary micturition, twitching of the mouth, lips drawn up, exposing the teeth, lips pallid, breathing catching and slow. 4.20.—Dead. In this instance unusual care was taken to perform the experiment with exactness. No air was allowed to enter the vein, and the ammonia was most carefully injected with the hypodermic syringe. The steps of the operation were most carefully carried out by Dr Ewart and myself.

The dog was bitten only once in the fore-leg. The poison did not manifest its effects so quickly, or in so marked a manner as in dogs bitten twice or thrice in the muscular part of the thigh, and this was purposely done that we might watch the progress of the action of the poison, and inject the ammonia at the right time. The ammonia was injected at 4.13 P.M., or in eighteen minutes after the bite. Convulsions came on immediately, and these were followed by complete muscular prostration; at 4.18, or five minutes later, twenty more minims of the ammonia were injected into the jugular vein; a repetition of the same phenomena followed, and the dog died, completely exhausted, at 4.20—that is, two minutes after the second injection, or seven minutes after the first, or in 25 minutes after the bite.

There can be no reasonable doubt that the injection of liquor ammoniæ into the external jugular vein in this case hastened, if it did not cause, death; and whatever other deduction may be drawn from the experiment, this is inevitable, that the proceeding is a dangerous one. In this case death occurred, in the first experiment the animal's life was in peril. The result is very different from that obtained by Professor Halford in his experiments, where he injected liquor ammoniæ not only into the jugular vein but into the heart itself, though I must at the same time confess that I cannot regard the latter experiment at all satisfactory or conclusive as to the beneficial effects of the ammonia. It proves the absolute necessity for many and most carefully-repeated experiments, before one can come to an absolute decision on a subject where there are probably several sources of error to be encountered.

EXPERIMENT No. 6.—The following experiments were made with the view of determining the influence of one poisonous snake on another. I have already made many experiments on this interesting question, and though so far the weight of evidence is in favour of immunity of the poisonous snakes to the poisons of their own species and those of others, yet I cannot regard it as a matter settled, but one about which there is still doubt. I must have further and more convincing proof before I can accept as a fact what I even now hardly believe, that a venomous snake, whilst it has the power of quickly destroying innocent snakes, has no power over its own or the other poisonous species. Of this, however, there can be no doubt, that the effect of the poison is much less active on a venomous snake than on an innocent one. A bungarus fasciatus, about six feet in length, was bitten about a foot from the tail by a full-grown fresh and powerful cobra, at 3.18 P.M.; again at 3.19; a third time at 3.20 P.M. All the bites were within a foot of the end of the tail. The object of selecting this part of the bungarus was to avoid the possibility of death being caused by injury to the viscera. The bungarus was then put into a cage. 4.40 P.M.—It seems quite well. 9 P.M.—Appears sluggish; the part of the tail below the bites appears partially paralyzed; on pressing the tail with a sharp-pointed instrument but little sensibility is manifested. *30th May*, 6 A.M.—Very sluggish; skin contracted into a longitudinal crease along either side of the body. 9.40 A.M.—Dead. The bungarus was a very large specimen, it was moulting at the time it was bitten; but still I think its death must be attributed to the influence of the cobra poison.

EXPERIMENT No. 7.—A full-grown spectacled cobra was bitten within a foot of the tail, by a daboia Russelli, about half-grown, but which was said to be fresh, and had been brought that day by the snake-men. There was some difficulty in making the viper insert its long slender fangs into the tough skin of the cobra, but it did so, finally, in several places. No evil result followed, and on 3d June the cobra was quite well.

EXPERIMENT No. 8.—A daboia Russelli was bitten by a fresh cobra near the tail, about the same time as that of the last experiment; the bites were several, and fangs well inserted; but no evil result followed, and, on the 3d June, the viper was unaffected.

EXPERIMENT No. 9.—Some cobra poison, taken from the poison-gland several months ago by Mr Sceva, had been kept, and had coagulated in the glass tube in which it was kept, into a white caseous-looking solid mass, with an intensely fœtid odour. Some water was mixed with this, in which it was only partially soluble. Ten drops of the opaque fluid were injected with the hypodermic syringe into a pigeon's thigh, at 4.20 P.M. No immediate result followed. 4.25.—Ten drops more of the same fluid injected as before. 9 P.M.—No apparent change in the bird. *30th May*, 6 A.M.—Lying down; wings drooping. 10.5 A.M.—Dead. This experiment shows that decomposition and coagulation of the poison does not, even after a long time, deprive it of its poisonous properties.

EXPERIMENT No. 10.—*5th June* 1869.—At 3.2 P.M. the right external jugular vein of a healthy dog was laid bare. Chloroform was then administered until the dog was insensible, though still whimpering with a peculiar cry. Forty drops of liquor ammoniæ, sp. gr. ·959, were carefully injected into the jugular. Immediately restlessness followed; limbs convulsed; howled loudly as though it felt acute pain, and when placed on the ground was unable to stand, the legs being powerless. 3.16.—Lying quiet. 3.17.—Pawing his mouth and face in a semi-paralytic manner; makes unsuccessful efforts to stand. 3.18.—Lying prone; unable to rise on his legs; paws the face; when put on his feet cannot stand; seems quite sensible and intelligent. 3.20.—In just the same condition; crawls, but is unable to stand on his legs. 3.21.—Sat up, but fell over again. 3.23.—Forty drops more were injected. It was doubtful this time whether the ammonia entered the vein; probably into the areola tissue about it. 3.25.—Forty drops injected this time certainly into the vein; the dog at once passed into a state of violent convulsion, and from that into a state of general tremor. 3.27.—Able to rise, and stagger a few steps. 3.30.—Recovering; the dog is and has been for some minutes profusely salivated. 3.33. —Running about the room; seems to be intelligent, but has peculiar nervous twitchings of the mouth and face. 4.30.—The dog has perfectly recovered; the crural vein exposed, and forty minims of liquor ammoniæ, sp. gr. ·959, injected, almost immediately succeeded by violent convulsions. 4.33.—Howling as if in pain or fear, legs paralyzed; struggles in the prone position, but cannot stand. 4.40.—Breathing hurried; puffing of buccinator muscle; twitching of orbicularis. 4.42.—Can walk but with a very staggering gait. 4.45.—Has recovered, but is weak. In this case the animal recovered; but each injection of ammonia was followed by violent convulsions, muscular tremors, and other paralysis to such an extent as to make it appear that life was in extreme danger. The effect produced was unsatisfactory, and suggestive rather of danger than safety.

EXPERIMENT No. 11.—3.37 P.M.—A large dog had the right external jugular vein laid bare; at 3.42, it was bitten in the right hind-leg by a spectacled cobra that had been in confinement for some time. The punctured leg and the neighbouring parts were lubricated with the snake's saliva. 3.47.—Sitting down; rises and walks with rather a tremulous gait. 3.48.—Lies down; deep inspiration; breathing hurried. 3.58.—Symptoms of poisoning not at all pronounced: there is reason to doubt the vigour of the cobra. 4 P.M.—Bitten again by a vigorous cobra in the same place; the leg immediately partially paralyzed. 4.1.—Uneasy; licks the wound. 4.2.—Whines, and is very restless. 4.3.—Head drooping. 4.4. —Breathing very rapid (100); tongue hanging out; whining; uneasy; lies down; is restless. 4.5.—Walks about whining; droops his head, and lies down; still strong on his legs. 4.8.—Can walk, but is evidently under the influence of the poison; staggers, pants, and droops his head. 4.10.—Forty minims of the liquor ammoniæ ·959 injected into the jugular vein; vomited; micturated violently whilst passing into a state of violent general convulsion. 4.12. —Stood up; breathing was hurried; is salivated. 4.15.—Shows weakness in the hind-legs; lies down; is purged; the symptoms of snake-poison becoming more marked. 4.17.—Injected forty drops more into the jugular vein; staggers, convulsed, and falls prone with the legs spread out. 4.20.—General paralysis; pupils widely dilated; tries to vomit; twitching of muscles. 4.22.—Twitching of all the muscles. 4.23.—Gasping; completely paralyzed. 4.24.—Catching respiration; involuntary micturition. 4.25 —Dead.

## THIRTEENTH SERIES.

Present—Dr Fayrer; Dr Ewart, Professor of Physiology; and Mr Sceva.—12th June 1869.

Experiment No. 1.—A fowl was bitten in the thigh by a spectacled cobra that had been kept in confinement for some weeks, had bitten before, and was therefore not fresh. The fowl was bitten at 3 P.M. At 3.1,50, fowl staggering; fifteen drops of strong Condy's solution, furnished by Messrs Scott, Thompson, and Company, were injected with the hypodermic syringe into the fowl's thigh. 3.2,50. —Fell down paralyzed. 3.4.—Lies almost dead; just breathing. 3.5.—Convulsed. 3.7.—Dead. Death occurred in seven minutes; but the bird was evidently unconscious after the first 2½ minutes. I could not recognise any good effect from the injection.

Experiment No. 2.—The left crural vein of a dog was exposed, ready to receive the injection. The dog was then bitten by a cobra in the right thigh at 3.20 P.M. The cobra was not fresh; it had been in captivity for some time, and had bitten before; but it was tolerably vigorous. 3.23.—Dog is excited and restless. 3.24.—Same condition; whining. 3.27.—Much excited; trying to break loose; is salivated; breathing hurried. 3.37.—Is beginning to show signs of the influence of the poison; is slightly convulsed; falls over. 3.37,30.—Injected sixty drops of liquor ammoniæ, sp. gr. ·959, into the crural vein; followed immediately by convulsive twitchings of mouth and limbs. 3.42.—Lying down, very low, almost motionless; irregular action of the heart; injected forty drops more of the liquor ammoniæ. 3.43.—No change; heart's action very feeble. 3.44. —Lies perfectly still, as though dead; no respiration; heart beats very irregularly; thirty-five pulsations in thirty seconds. 3.45.—Dead.

Post-mortem examination of body at 4.20 P.M.—Lungs, pallid; no congestion. Heart, right side much distended with black clots. Left ventricle contained a little dark fluid blood. The viscera generally were pallid; but the liver was somewhat congested. Brain, —cerebral substance free from congestion; vessels on surface slightly distended with blood. The blood generally coagulated firmly.

Experiment No. 3.—The external jugular vein of a dog was exposed at 3.34 P.M.; forty drops of the liquor potass. permanganat. (Condy's) were injected into the vein at 3.35. No effect produced at the time; the dog did not appear to take any notice of the injection. 3.40.—Dog apparently not affected. 3.45.—Seems rather depressed, but this is not marked; it may be fear. 3.48.—Bitten by a large cobra (not fresh, for it has been some time in captivity, and has bitten before) in the thigh. The fang punctures were at once washed with the Condy's solution, which was well rubbed in. 3.49

—Bitten leg partially paralyzed. 3.50.—Lying down; when raised can stand, but quickly lies down again; is quite intelligent. 3.51. —Droops his head. 3.52.—Sixty more drops of the fluid injected into the vein. 3.54.—ʒii. injected into the bowel; is able to sit or stand, but is very weak. The injection of the Condy was not followed by convulsions as in the case of the liquor ammoniæ. 3.58. —Lies down; head falls over; breathing hurried; rises and staggers a little, and sits down again. 4 P.M.—Lies sluggish and dejected; can walk when raised, but staggers and soon sits down again. 4.5. —Can still stand and walk with staggering gait. 4.7.—Lying down, but gets up and walks a few steps; head drooping, and look dejected; twitching of muscles generally. 4.8.—When put on his legs, can still stand; breathing hurried; coat staring. 4.12.—Forty more drops injected into jugular vein. Slight twitching of muscles generally; lies down on his side, cannot rise; limbs paralyzed; pupils dilated; slight convulsions of extremities, and muscular system generally; breathing catching and rather slow. 4.22.—Motionless; heart still beats fifty in the minute; no respiration. 4.24.—Heart still felt. 4.25.—Dead. Bitten at 3.48, dead at 4.25—in thirty-seven minutes. Death occurred in about the usual time, and with the usual symptoms, in which it occurs, when a dog is bitten by a cobra. I do not believe the effects of the poison were in any way influenced by the permanganate.

Post-mortem at 4.40 P.M.—Lungs much collapsed and very pallid. Both sides of heart full of fluid blood; great vessels distended. The blood coagulated firmly when let out of the heart and vessels. Abdominal viscera not so pallid as in the other dog. Brain vessels on surface full of blood; cerebral substance pale, scarcely any puncta.

EXPERIMENT No. 4.—A fowl bitten by a cobra (not fresh) in the thigh, at 4.45 P.M.; twenty drops of liquor ammoniæ having previously, at 4.43, been hypodermically injected into the thigh, no apparent effect produced by the ammonia thus injected. Bitten by the cobra at 4.45 P.M.; in thirty seconds it was in violent convulsions. 4.46.—Twenty more drops injected. Dead before it could be placed on the ground.

EXPERIMENT No. 5.—The following experiments were tried with the object of again testing the effect of the cobra poison on itself, or on another cobra. A cobra was bitten sufficiently near the tail to avoid the chance of injuring any of the viscera, by another full-grown, fresh, and vigorous cobra. The snake bit deeply, plunging its fangs twice or thrice into the flesh of the other cobra, which was then put aside in a separate cage and watched.

13*th*, 6 A.M.—No change.—7 P.M. Do.—14*th*, 6 A.M. Do.—6 P.M. Do.—15*th*, 6 A.M. Do.—The snake evidently has not suffered.

EXPERIMENT No. 6.—A vigorous and fresh full-grown cobra, with one occellus in the hood (*keautiah* of the natives), was made to bite himself three or four times near the tail at 4.48 P.M. He bit himself quite as freely as he would have bitten another snake, brought blood, and smeared the surface with poison; put into a cage and watched. Was not the least affected on the 15th, three days after the bite. These two experiments, which were very carefully performed, tend to prove that the cobra cannot poison itself or its own species.

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Present—Drs Fayrer, Ewart, Waller, and Mr Sceva.—
19th June 1869.

EXPERIMENT No. 7.—A full-grown cat was bitten at 2.43 P.M. in the right thigh by a large daboia. The snake had been long in captivity, and was therefore most probably not vigorous, though active and vicious. 2.49.—Twitching of the muscles; restless. 3.12.—The cat appears to be unaffected. 3.15.—The daboia again made to close his jaws on the cat's thigh, though evidently unwilling to bite. 3.44.—The cat seems sluggish, and is evidently now feeling the poison. 3.45.—Bitten by a cobra on the thigh. 3.46.—The cat is unsteady in its gait; breathes hard; looks distressed. 4.7.—Cat is restless; tries to rise, and staggers. 4.10.—Tries to stand, but falls over. 4.18.—Respiration very hurried; cannot stand; twitching of the muscles. 4.24.—Lies quite paralyzed; pupils dilated; breathing hard. 4.27.—Convulsed. 4.30.—Dead. The cat was bitten by the daboia at 2.43 P.M., and again at 3.15. The snake was old and feeble; it has been in captivity for more than six months, during which time it has touched neither food nor water, and yet it was active and vicious, hissing fiercely at any one who came near it; that it was capable of secreting poison was evident in later experiments. The cobra bit at 2.43, and death occurred at 4.30; that is, in one hour and forty-seven minutes. The cobra was also not fresh, and its poison was weak, perhaps scanty. There was nothing whatever in this experiment to make us think that the cobra's poison proved antidotal to that of the daboia; on the contrary, it rather expedited death.

EXPERIMENT No. 8.—A fowl was bitten in the thigh by the same daboia at 3.40. The snake would not bite until his jaws were closed on the bird's thigh. 3.44.—The fowl, whilst walking about with rather a sluggish gait, suddenly sprang off the ground, and fell over in convulsions. It was immediately bitten in the thigh by a cobra. It continued unconscious and convulsed, and was dead at 3.46; that is, in six minutes after it was bitten by the viper. Had this viper been fresh, the bird would probably not have lived one minute.

EXPERIMENT No. 9.—Another fowl bitten by the same daboia at

3.56 P.M. 3.58.—It limps; has a depressed look, and its comb droops. 3.59.—Bitten by a cobra in the thigh; lies down. 4.2.—Slight convulsions. 4.3.—Comb livid; convulsed and unconscious. 4.5.—Dead,—in nine minutes. These three experiments, I think, dispose of the question of the poison of one family of venomous snakes being antidotal to the other. In these cases, the viper was old and exhausted, and yet his poison was deadly. The poison of the cobra, which was also a partially exhausted snake selected on purpose, had no counteracting effect. The only thing proved is the terribly deadly nature of the daboia, which after such long confinement, without food or water, yet retained the power of causing death.[1]

These experiments were made in reference to certain suggestions that have appeared in the journals, but not with any expectation on my part that any other result than that which occurred could take place. The poison of the deadly snakes, of whatever family, kills by paralyzing the nerve-centres, and it appears as reasonable to expect prussic acid to prove antidotal to aconite, as the cobra poison to be so to that of any other form of venomous snake.

EXPERIMENT No. 10.—A fowl was bitten by the same daboia in the thigh at 4.7 P.M. 4.10.—Sitting down; looks sluggish. 4.15.—Rises and runs about, but is lame. 4.27.—Walks, but is very lame on the bitten leg. 4.45.—Still walks about, but is sluggish and lame, and looks very dejected. The fowl, after this, began to recover, and on the 21st, two days later, was quite well. The snake was evidently all but quite exhausted when he bit this bird.

EXPERIMENT No. 11.—The external jugular vein of a dog was exposed at 3.6 P.M., and four drops of cobra poison were injected; at least one drop was lost, the other three entered the vein. 3.10.—Dog looks dejected, and ears drooping; he lies down. 3.33.—Beyond being sluggish, no symptoms of poisoning. 3.46.—Very sluggish; lies down. 3.47.—Liquor ammoniæ, sp. gr. ·959, sixty drops injected into jugular vein; dog lies quiet. Heart beating rapidly; respiration very feeble. 3.54.—Heart's action very rapid; breathing rapid; muscular twitchings. 3.57.—Injected sixty more drops into the vein; muscular twitchings continue. 3.59.—Dead. Poison injected at 3.4; death at 3.59. Death in fifty-five minutes. The quantity of poison was very small, from a weakened snake; no effect was produced by the ammonia.

EXPERIMENT No. 12.—The jugular vein was exposed in a dog; it was then bitten in the thigh by a fresh cobra at 3.27. 3.28.—Staggering; excited, springing; howling violently; and trying to break the cord by which it is tied. 3.29.—Quiet; sitting down. 3.30.—Head drooping. 3.33.—Lying on its side, slightly convulsed; sixty drops of a solution of quinine, of the strength of one grain in eight drops, were injected into the jugular vein. 3.34.—The dog lies on its side, still slightly convulsed. 3.35.—Dead. Bitten at 3.27; dead at 3.35, in eight minutes. The quinine evidently did no good.

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[1] The daboia was not intentionally deprived of food or water; it would take neither.

EXPERIMENT No. 13.—Equal parts of cobra poison and liquor ammoniæ, sp. gr. ·959, were mixed together, and fifteen drops of the mixed fluid were injected with the hypodermic syringe into a pigeon's thigh at 4.30 P.M. Pigeon crouched immediately; at 4.31 was unable to stand, the beak resting on the ground. 4.32.—Convulsed; peculiar convulsive movements of the tail continuing. 4.32.—Dead. Injected at 4.30; death at 4.32, in two minutes. This experiment is very unfavourable to the theory of the antidotal action of liquor ammoniæ.

EXPERIMENT No. 14.—The external jugular vein of a large and powerful dog having been exposed, ten (10) drops of fresh cobra poison were injected into it at 4.24 with the hypodermic syringe. 4.24,30.—The dog staggered, was convulsed, and fell over, foaming at the mouth. 4.25.—Violently convulsed, but with no outcry or sign of suffering. Sixty drops of liquor ammoniæ, sp. gr. ·959, injected. Dead. Death occurred in about 70 seconds; showing the frightful virulence of the poison when it finds entry by a large bloodvessel.

How can such a death be explained otherwise than by exhaustion of the nerve-centres? Any theory of blood-change is surely totally inapplicable here.

Present—Dr Fayrer; Dr Ewart, Professor of Physiology; and Dr Sceva.—26th June 1869.

EXPERIMENT No. 15.—A pariah dog was bitten in the fore-arm by a cobra (*kalla keautia*) at 3.2 P.M. A ligature had been thrown round the limb above the bitten part, which was immediately tightened; a pointed steel, heated to a red heat, was then, at 3.3 P.M., inserted into the punctures, and the wounds were thoroughly cauterized. 3.7.—The dog is restless, and is apparently under the influence of the poison. 3.12.—Staggers as he walks. 3.14.—Forty drops of liquor ammoniæ, sp. gr. ·959, diluted with three parts of water, were injected into the jugular vein. 3.17.—The dog runs about excited; he was partially convulsed during the injection of the ammonia; now sits up, and then falls over backwards; breathing quickly. 3.20.—Lies down; is salivated. 3.27.—Sits down; paws the air, muscular twitchings. 3.38.—Lying on his side; convulsed. 3.44.—Lies paralyzed; heart still beats, but no respiration. 3.45.—Dead.

Notwithstanding the ligature, which was tightened immediately, the actual cautery, which also immediately followed the cobra's fangs, and the injection of ammonia into the venous circulation, the snake-poison proved fatal to a full-grown dog in forty-three minutes.

EXPERIMENT No. 16.—A dog was bitten by a fresh cobra (*kalla keautia*) in the fore-arm at 3.38 P.M.; a ligature was immediately tightened round the limb above the wound. The actual cautery was at once applied, until the fang wounds and the adjacent parts were completely disorganized. 3.42.—The dog is sitting, but reels as though he would fall over. 3.49.—Rises and walks with a staggering gait. 3.54.—Sits down; attempts to get up, and falls over backwards. 3.57.—Is convulsed; falls over, cannot stand; hurried breathing. 4.4.—Cannot move; lies paralyzed; heart still beating; respiration almost ceased; pupils widely dilated. 4.6.—Lies on his side; convulsed. 4.12.—No respiration; but heart still beats feebly. 4.13.—Dead.

In this case also, notwithstanding the ligature which was applied as tightly as two persons could pull it round the leg, and the deep and thorough actual cauterization immediately after the bite, the snake-poison found entry into the system, and proved fatal in thirty-five minutes. The dog was much smaller than that of the first experiment.

Nothing, it seems to me, can more strongly demonstrate the extremely subtle and virulent nature of the cobra poison than those experiments; nothing, I think, is more significant of the improbability of anything proving to be an antidote. If the poison find entry into the bloodvessels, and be carried to the nerve-centres, I am inclined to believe that nothing can prove of any avail, excepting in those cases where the bite is imperfect, the quantity or the quality of the poison diminished or deteriorated, or the snake itself is young, weak, exhausted, or is one of less poisonous family; such, I believe, are the only cases in which recovery occurs through the inherent vigour of the animal or person bitten, perhaps aided by stimulants and excitement. The favourable result is attributed, and naturally enough, by those who do not understand the *modus lædendi* of the venom, to the treatment and the so-called antidote. That we can aid in such recoveries, and that we may do much to help the sufferer through the troubles arising from general disorder and secondary blood-poisoning, I have no doubt; and I would offer every encouragement to all to persevere in their attempts; but I must state my conviction, that nothing that can properly be called an antidote to cobra or viper poison exists; and the more this is known the better, for mistaken notions on such an important matter can only do harm, and may be the cause of losing rather than of saving life.

My belief is that, if an animal, and probably a man, be fairly bitten by a fresh and vigorous cobra or daboia, it or he will inevitably succumb, unless some immediate and direct method of arresting the entry of the poison into the circulation be practised.

That such may be done I will not deny; but the two experiments just recorded, performed with the greatest care and speed by two surgeons accustomed to such operations, show that, at the least, it is very difficult. The moment of time that intervenes between the injection of the snake-poison by the powerful maxillary muscles through the tube-like fang into the minute bloodvessels of the part, and the application of the ligature and actual cautery, is sufficient to allow of the entry of the poison into the circulation, and this reaching the nerve-centres, even in a small quantity, may prove fatal. The ligature is evidently very unreliable, when applied to large parts of the body, such as the limbs; for it is almost physically impossible to compress the part so tightly as to stop the circulation; and unless this be done to the depth of the penetration of the snake's fangs, it is obvious that it can only be of very partial effect in preventing the entry of the poison. On a finger or a toe the ligature might be of more service, as the smaller part might be thoroughly strangulated; but unless the ligature were applied immediately, it is obvious that it would be useless even there, for the poison would have already entered, and be on its course towards the nerve-centres. How quickly this occurs is proved by those experiments in which the poison was injected directly into the jugular vein. What took place there, with the hypodermic needle inserted into the jugular vein, has its exact counterpart in the case of the cobra's fang, inserted, as it must be, when it penetrates a vascular part into the minute veins.

The same may be said of the actual cautery. Unless the hot iron enter the puncture directly after the fang has been withdrawn, the poison is already far on its way towards the centre, and the burning, though it destroys the tissues and such of the poison as may not yet have entered the circulation, can have no influence on that which is already beyond its reach. But as the ligature, if tightly and quickly applied, and the actual cautery, if promptly and thoroughly inserted, must limit to a certain extent the entrance of the poison, both should be had recourse to as speedily and efficaciously as possible, in the hope that the amount of poison left to find, or that may have already found, its way into the system, may be less than is sufficient to cause death.

To conceive of an antidote, in the true sense of the term, to snake-poison, one must imagine a substance so subtle as to follow, overtake, and neutralize the venom in the blood, or that shall have the power of counteracting and neutralizing the deadly influence it has exerted on the vital forces. Such a substance has still to be found, and our present experience of the action of drugs does not lead to hopeful anticipation that we shall find it.

But I repeat, that where the poisonous effects are produced in a minor degree, or when the secondary consequences are to be dealt with, we may do much to aid the natural forces in bringing about recovery. This is not, however, what is meant by an antidote.

EXPERIMENT No. 17.—A large and powerful dog had the right external jugular vein exposed. Twenty drops of a mixture of fresh cobra poison, taken from the snake the same day, one part, and liq. ammoniæ, sp. gr. ·959, forty drops or two parts, were then injected with the hypodermic syringe into the vein. The time of the insertion of the fluid was 4.27,30. The effect was instantaneous; the dog struggled, howled, and was convulsed on the table: he was immediately released and placed on the ground, but was already almost unconscious and convulsed. He made an effort to rise on his legs, and fell prone on his belly. Within one minute respiration had ceased, though the heart's action continued faintly. This ceased, and at 4.30 he was quite dead. The action of the poison with the ammonia was frightfully rapid in this case. Death occurred in two minutes and a half, complete unconsciousness within a minute; and only by the faint beating of the heart, which only continued for two and a half minutes, was any sign of life manifested. This surely is fatal to the theory of ammonia injected into the circulation being of any benefit in snake-poisoning. In this case the poison and the so-called antidote were injected synchronously; the result was almost instant death.

The experiment was performed by Dr Ewart and myself with the greatest care, and certainly no air entered the vein.

EXPERIMENT No. 18.—The cobra that bit the dog in the first experiment bit a fowl in the thigh at 3.46 P.M. The bird immediately began to limp, and then crouched, and then fell over. 3.47.—Head fallen over, beak resting on the ground. 3.49.—Convulsed; dead. Death occurred in three minutes.

A second fowl was bitten by the same snake, at 3.50 P.M., in the thigh. 3.51.—Drooping his wings. 3.55.—Sits down, beak resting on the ground. 3.57.—Is convulsed. 4.—Dead,—in ten minutes.

A third fowl bitten by the same snake in the thigh at 3.51 P.M., shortly after drooped. 3.58.—Convulsed. 4.2.—Dead. Dead in eleven minutes.

A fourth and larger fowl bitten in the thigh by the same cobra at 4.3 P.M. 4.8.—Crouching; wings spread out; gets up; tries to run, and falls; head droops, beak resting on the ground. 4.10.—Convulsed. 4.17.—Still convulsed; comb livid. 4.20.—Dead,—in seventeen minutes.

A fifth fowl bitten in the thigh by the same cobra at 4.13 P.M. 4.20.—Crouches; comb drooping. 4.24.—Head drooping; resting on beak. 4.29.—Quite paralyzed; convulsed. 4.35.—Dead,—in twenty-two minutes.

A pigeon was bitten in the thigh by the same cobra at 4.37 P.M. 4.47.—The pigeon is drooping, and when he stands, it is on one leg, and then falls over again. 5.22.—Dead,—in 45 minutes. This was the ninth animal bitten by the cobra in rapid succession, and still it is apparently not quite exhausted.

A sixth fowl bitten in the thigh by the same cobra at 4.32 P.M. 4.35.—Crouching. 4.47.—Staggers. 5.45.—Lying down insensible. 6.5.—Dead,—in 93 minutes.

A seventh fowl bitten by the same cobra in the thigh at 4.34 P.M. 4.37.—Crouches. 4.47.—Seems sluggish, and limps. *27th June*, 5.30 A.M.—Lying down, and eyes half closed; unable to walk, *28th June*, 6 A.M.—Is recovering; walks sluggishly and limps, but is evidently regaining strength.

The object of this experiment was to test the extent of power possessed by the cobra. It destroyed one dog, six fowls, and a pigeon in rapid succession; but the intervals between the bite and the death of each were prolonged, showing the gradual diminution of power at each bite. The seventh fowl poisoned was only slightly so, and recovered.

The cobra was neither a very large nor a very vigorous one, and yet how deadly! Eight creatures destroyed by a rapid succession of bites. The experiment proves that the snake becomes weaker by biting, until quite exhausted.

EXPERIMENT No. 19.—A daboia was bitten by a fresh cobra (*kalla keautia*) near the tail, sufficiently far from the viscera. The scales were previously scraped off. The snake bit fiercely and repeatedly at 4.54 P.M. 6 P.M.—No change. On the 28th June, at 6 A.M., there was no change.

The object of this experiment was to repeat the test of the influence of the cobra poison on the viper. The result tends to show that it is innocuous.

(*To be continued.*)

# Part Second.

## REVIEWS.

*Lectures on Obstetric Operations, including the Treatment of Hæmorrhage, and forming a Guide to the Management of Difficult Labour.* By ROBERT BARNES, M.D., etc. London: John Churchill & Sons: 1870.

WE have several books in the English language on operative midwifery, and several in other languages. Dr Playfair's volume on obstetric operations is a handy and commendable work, and, with the exception of Barnes's treatise, the latest. Dr Barnes is more ambitious than any of his English predecessors, and more original in some senses. We have casually observed the appearance of all the substance of this book in weekly journals and otherwise, and have been gratified by the zeal of Dr Barnes, his cleverness, his literary power, his ingenuity. But Dr Barnes now puts his labours into a handsome octavo form; has reconsidered his flying sheets for the weekly journals, has published them in a mass; and we have now, as we are called upon, much pleasure in noticing this latest and most important addition to British obstetric literature.

We have read the whole work without tiring. It is, on the whole, well written. It is scarcely in the lecture style, although the occasional occurrence of the form of direct address shows that it is written really in the form of prelections to students.

We have a decided preference for the second half of the book—on the induction of labour, and on hæmorrhage. This part is far more accurate, scientifically regarded, than the first part; and, besides, the practical directions are more nearly in accordance with sound and standard therapeutics.

We may point out a remarkably able statement (p. 408) of the reasons for throwing overboard the treatment of placenta prævia by separation of the entire placenta; a statement which, while it presents nothing new to experienced and thoughtful observers, is a very good and practical account of why the treatment cannot be expected to gain confidence—of why it has gained no confidence. But, when we turn to further disquisition on the allied subjects, we find nothing equally good in favour of Barnes's own method of separation of the placenta, not entirely, but from the orificial zone. This, however, is one of the author's hobbies, and he may be excused for esteeming it more highly than other people. On the whole, we have scarcely a fault to find with Barnes's account of the treatment of placenta prævia, if we just omit that part of it which he prizes most, separation from the cervical zone. The treatment, speaking generally, of labour complicated by placenta prævia remains where Rigby put it:—Deliver as soon as possible, and generally by turning. The real deficiency of this rule is observed in cases of hard and small os uteri. Then the rule is applicable theoretically, but it cannot be applied practically. The os is too small for immediate or timeous turning. Separation of the placenta from the cervical zone, the plan of Dr Barnes, is ingenious: that is all. Bipolarity of turning is mere trifling; there is no difficulty there. What is wanted is dilatation of the cervix. This is well pointed out by Barnes, and he has the merit of supplying us with one good means of effecting the desired object—his India-rubber dilators. But several authors, before Barnes, have felt and expressed the want, and described means of securing the object.

We have said that, scientifically, the last chapters of the book are better than the first. But we find many points for criticism. Velamentous insertion of the cord is erroneously called velamentous placenta. The placental site *and the cervix* are said to be the sources of hæmorrhage or flooding. The cervix is said to be a source, because it is flaccid and has been distended and bruised. This is a manifest error. Flooding proceeds from the placental site, not from the anatomical cervix however flabby or bruised. We find such a sentence as the following, which elicits only doubt and the question,—How do you know? "In the case of fundal attachment, the separation begins at the centre of the placenta, and extends to the circumference" (p. 440). The same doubt and question arise when we read of the fœtus in certain cases. "It perishes of asphyxia, arising from the *mother's loss of blood* and collapse, and the partial or complete detachment of the placenta" (p. 438). The same doubt and question again, when we read of ergot (p. 437): "If absorbed, it is injurious by adding to the prostration." The expression of doubt passes into assertion of error, when we read that the site of the rent of the membranes proves the existence of placenta prævia. "This is," says our author, "afterwards proved by observing that the rent in the membranes is within an inch or two of the edge of the placenta" (p. 436). Has Dr Barnes never considered the production of a caul? Has he often observed the site of rupture in cases of placenta not prævia? We might cite other such questionable statements, given as if incontrovertible, and therefore prematurely made parts of a scientific argument.

We go to page 429, where we find a formal statement of doctrine in regard to placenta prævia, and here we find great deficiency in the precision which the mode of writing in propositions demands. Scarcely one of these propositions will endure rigid examination. In proposition 6, for example, *necessarily* and *commonly* are used as if interchangeable terms. Proposition 7, with the qualification "at the posterior part," is without a particle of foundation. Proposition 8 is in a like condition. Proposition 5 is the chief one; it is to the effect that it is the case, or, Barnes thinks, that detachment and flooding ought to stop when the prævia placenta is partially separated, the partial detachment being from the lower zone. But Dr Barnes nowhere proves that the bleeding does stop, the evidence that is desiderated. Besides, his physiological reason that it should stop, whatever that physiological reason may be, is almost certainly inconsistent with some of his physiological reasons against the complete detachment of the placenta. Further, he will find it difficult to prove his proposition 5, if he keeps in mind his therapeutical proposition 6 (p. 432); to which proposition also we do not assent.

Though these remarks are critically just and true, we must add that no one, whether practitioner or student, can read his chapters on hæmorrhage without interest and benefit. Yet he will be amused to find, even in the best practical paragraphs, such advice as to apply ice to the back of the neck in post-partum hæmorrhage!

The excellent section of the book, on induction of premature labour, is defaced by a passage which is inconsistent with the rest of it:—"That it is just as feasible to make an appointment at any distance from home to carry out at one sitting the induction of labour, as it is to cut for the stone" (p. 352).

We now come to the larger and first part of the book. It is on the great midwifery operations. We have already said that it is not so satisfactory as the rest. It is too ambitious in the practical parts. It is too dogmatic. In science it is very imperfect, and has many errors.

The excellent, and, in its own place, very valuable bipolar method is worked to death, both scientifically and practically. Our author praises the operative turning powers of old accoucheurs, and says we cannot equal them. The justice of this praise, that has been given by several recent authors, we much doubt. At present we only say, that such ascription of praise can scarcely be just, if the bipolar method of turning is so great a discovery as Barnes asks us to believe.

We divide obstetricians into two kinds: 1. Those who meet with few novelties or difficulties; 2. those who seem to meet with nothing but novelties and difficulties. These last are always telling extraordinary stories. Among these latter we are almost disposed to class Esterlé or D'Esterlé (Barnes is very fond of quoting Italians whom no one ever heard of, and who owe much to our author). If any one wants to see a poor Italian convicted of talking "at large," let him read pp. 174 and 175. Unfortunately for Lazzati, and perhaps even in some degree for Barnes, they are nearly in the position of Esterlé or D'Esterlé. This gentleman finds, during pregnancy, a *large number* of shoulder cases, and performs head-turning before labour. Collins, in above 16,000 cases of labour, had in all (without head-turning) only 40 cases of transverse presentation. Esterlé has observed the "*frequent* occurrence of spontaneous version in the eighth and ninth months of gestation." Where did Esterlé practise, and what does *frequent* mean here? Esterlé "observed that a *large number* of shoulder-presentations in the last two months, if left to themselves, were converted into natural presentations, either on the approach of labour or after the beginning of labour." Such is Esterlé's statement. It is at least intelligible. His account of the cause of the change of presentation is something or other; we cannot say it is intelligible. "The most efficient cause of spontaneous version," he says, "is the combined action of the movements of the fœtus and of its gravity, the centre of gravity not being far from the head. The extension of the feet (!) must drive the breech away from the uterine wall as the feet strike it, and so the head is brought nearer to the brim." The plan of producing the desired change of position is truly ridiculous. If, instead of it all (p. 175), it had just been directed gently to push the child into the right position, and hold it there, then we should have at least had sense instead of Esterlé's bombastic making of difficulty.

Dr Barnes has a great power with his pencil, and he illustrates his book freely by drawings, which are generally vigorous, and answer the purpose for which they are intended. But some are of doubtful accuracy; see p. 218. As a whole they are not so good as those of Churchill's manual.

We intended to enter upon the forceps chapters, which present many features that might easily be shown to be doubtful or erroneous. Some parts appear, indeed, to be contradictory to others.

We also wished to enter into details on Barnes's account of spontaneous evolution. Here he becomes quite enthusiastic, not only about the evolution, but also in defence of the worthy Denman, and he signally fails. He seems blind to the facts that Denman's cases are inconsistent with Denman's theory, and support Douglas, and that, while Denman's facts are consistent with Barnes's spontaneous evolution, his theory of the same cases is not adopted by Barnes. With the exception of the defence of Denman, Barnes's chapter is a piece of good obstetrical writing, well worthy of study.

No one denies that spontaneous version may occur. Denman describes it imperfectly; but Denman describes it as the regular mechanism of what Barnes calls spontaneous evolution, what Douglas calls spontaneous expulsion. The class of cases of shoulder-presentation, where the waters have been long discharged, and the shoulder is deep in the pelvis, which are spontaneously terminated, obey the mechanism described by Douglas as that of spontaneous expulsion—not that described by Denman as that of spontaneous evolution, which is the same thing. This is Denman's error. But Denman is not so far wrong as Barnes helps to make him, for Denman describes the mechanism not as he observed it, but as he fancied or *presumed* it might be.[1]

The last chapters on which we shall make a few remarks are devoted to turning. None show Dr Barnes's literary powers, his generous impulses, his ingenuity, and his obstetric knowledge, to better effect. We strongly recommend their perusal to that class of readers who have devoted some time to the thorough study of obstetrics, and who have enjoyed as much experience at least as is necessary to have made them feel the difficulties of a few difficult cases. Such cannot read them without advantage. Yet here, as elsewhere, in regard to this book, we feel that we cannot criticize without our objections to some parts or points coming strongly forward. We may say, for instance, that we do not approve of Barnes's advice to seize a knee instead of a foot or feet in turning, nor of his advice to seize the farthest knee in difficult turning. This last advice he supports with great ingenuity; but it is the ingenuity of a special pleader. It would be easy, if one had the literary power of our author, to make as good an argumentative display in favour of seizing the near knee. The question is to be settled not by reasoning, which cannot reach the whole circumstances and make them available, but by practice; and we recommend practitioners to seize the near limb.

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[1] It has amused the reviewer, after writing the above remarks, to find them tally exactly with Denman's own explanation and semi-admission of his being mistaken in his theory of spontaneous evolution. In a letter on this subject, Denman says—"The fact is a distinct question; the manner of the evolution another. For the former I am not any longer answerable. It stands on another testimony; but I certainly have remained responsible for the explanation of the manner, and to defend this I am not very solicitous. Yet I may observe that my explanation is not given in positive terms, beginning with 'I presume,' leaving it as an opinion for future proof or disapprobation. If there be an error in the explanation, others may also err in their opinion."

The chapters on turning contain much that is not necessary. For example, we have a discussion as to the cause of the position of the fœtus in utero, which might have been unobjectionable (though not necessary) if it had not been so very imperfect, and we believe misleading. The statements of mechanism, whether of cranial or shoulder presentations, are much better.

Dr Barnes, of course, treats the great question, old and still young, of turning in contracted pelvis, its scope and its position in relation to the long forceps operation in the same or similar circumstances. This is one of the most important and one of the most difficult questions in midwifery, and very much has yet to be done ere it can be settled. Dr Barnes contributes his words and experience, and goes in favour of turning, not to the rejection of the long forceps, but giving delivery by turning the higher place as a more powerful means of delivery applicable to many cases in which the forceps may be used; but also applicable to a class of cases in which the forceps is not available, on account of the great contraction of the brim. We are averse to expressing dogmatically any opinion on this subject; only, we may say, that it is our impression that the current of soundest obstetrical judgment is at present running in favour of the forceps rather than of turning. To Dr Barnes's chapters on this great topic one objection is, that they are behind the day. He might have, with much advantage, omitted to read and study a great deal, to which he has not only paid attention, but to which he has called the attention of his readers. At the same time, he has omitted to notice much that is essential to the advance of the subject to a new stage. Such a sentence as the following tells a great deal—"And surely," says Dr Barnes, "no one can doubt that the traction-power, and, therefore, the compressing power, acquired by pulling on the legs and trunk, is infinitely greater than can be exerted by the strongest forceps" (p. 280). There is here an extreme neglect of attainable exactness in introducing the word "infinitely," and an extreme neglect of the proper mode of searching for truth in this question. To use the word *infinitely* is very wrong, when we positively know that the difference between the two powers is not many pounds. To use this word is very wrong, when, instead of such infinite or indefinite talk, we can measure and state definitely. To use this word is very misleading, when we know that the power required to pull the child to pieces is not great. But, above all, the whole sentence is wrong in theory and practice; for while the power by pulling the child is limited by the strength of the neck, and the limit is easily reached, the power of the forceps can be indefinitely increased, and, at least, so as to exceed the strength of the child's neck. All this has been already well demonstrated by various authors. If they had not shown it, it would be the first thing they have to do, if they wish to give this great question of turning an impulse. In the same manner, it is mere idleness to talk of the shape of the head, its wedge-like construction, the comparative ease of compressing it, and making it collapse by traction from below. All this is, as we have said, mere trifling, when the question (if it has not already been) may be easily and finally settled, not by ingenious talk, but by the method of observation and experiment—a method which has done much for midwifery, and is destined to do very much more, not only in discovering truth, but also in destroying error. We fear that the settlement of this point by actual experiment does not accord with the notions of Barnes and others founded on very imperfect investigations and ingenious hypotheses. A few fœtuses, a couple of blocks, and a forceps, are all that is required, and that have been used for the experiments.

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*The Pathology and Therapeutics of Mental Diseases.* By J. L. C. SCHROEDER VAN DER KOLK, Professor of Physiology in the University of Utrecht. Translated from the German by JAMES T. RUDALL, F.R.C.S. Ireland. London: John Churchill & Sons: 1870.

IT is refreshing to take up a treatise on insanity which does not commence with a long dissertation on metaphysics. No doubt, it must be a great temptation to authors of such books to air their ideas on this subject, and probably it may be of interest to the metaphysician to read the lucubrations of men who have had under observation morbid mental manifestations, but to the student of medicine it is of far greater service to have before him a book containing more substantial food; this he will find in the first part of Schroeder van der Kolk's book now before us. This great physiologist has brought to bear on the pathology of mental disease his own vast stores of knowledge, and collated with them the results of the observations and experiments of other foreign authors in a fair and inquiring spirit, and has succeeded in building up a theory which we humbly conceive will stand the test of time.

The author is necessarily a materialist—a physiologist cannot be otherwise—but he is a materialist of the right sort. He inculcates fully the connexion of mind with matter, and their mutual reaction, but he insists most strongly on the existence of a higher and utterly immaterial essence in the human constitution.

This is peculiarly evident in the section in which he speaks of the action of the cells in the cortical substance (p. 13). He instances the result of certain influences which accelerate or retard the circulation in the brain, and so modify or excite the action of the cells, producing mental phenomena involuntarily to the *ego*. He says, "But, we may ask, does not this theory lead to the grossest materialism, and will not our mind through it be degraded to the level of a mere cell life? By no means. By that interpretation, during which I have, as much as possible, truly followed the course of nature, the independence of the individuality of the mind remains, according to my opinion, in the clearest way secured. For, during the time in which those confused and intricate images pass before our mind, we may at will firmly retain one of them, so as further to embellish it quite according to our liking. That proves, then, that a still higher faculty is active in us, which can receive and perceive those impressions, but is also at the same time in a state independently to interfere and to govern the orderless mass. We have a telegraph office, but at the same time also a telegraphist who voluntarily and independently exerts influence. The involuntary delineation of those images is an organic operation of the brain: we feel that the images are presented to us, that they are not our own individuality, and that they exist as objects outside of us. Our will, on the contrary, belongs to our individuality; it is not product of the body, but of the mind; it is subject. The materialist will compare us to a telegraph office without a telegraphist, to an automaton who only blindly acts, without order, without will, without understanding and judgment—to an uninterrupted dreamer, who cannot perceive his own dream. *Free will belongs to the higher individuality, it is an attribute of the soul.*"[1]

The first part of this book cannot be too strongly recommended to the student of Cerebral Anatomy and Physiology. It tersely yet clearly puts forward all that was known at the period of its composition, and what has since been produced only tends to support its theories. They require deep study and careful consideration; they are too elaborate to bear compression—injustice to their importance would result. The sections treating on the action of the cells in the cortical substance, and on the influence of the circulation in the production of morbid mental manifestations, are eminently suggestive. The general principles laid down will enable the reader to approach the mysterious puzzle of cerebral pathology, guided by a very bright physiological beacon.

In reviewing the latter half of this work, we are desirous of showing how far the author has advanced beyond others in the most important subject of classification of diseases in which mental symptoms are predominant; how far he has succeeded, and how far he has failed.

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[1] Italics are our own.

Skae, in his address to the Medico-Psychological Association in 1863, entered a strong protest against the old psychical classification, and suggested a system of nomenclature which is gaining ground daily. It cannot be considered perfect, nor indeed did its author claim for it the attribute; it consists of the arrangement of mental disorders in their natural orders or families, grouped in accordance with the natural history of each. The fault of this system is, that natural orders were sought to be formed before primary grand divisions, founded on pathology, were established. Skae began from below; our author, as we will show, has begun from above.

Griesinger took high ground when he proposed to consider all diseases of the nervous system as "one inseparable whole, of which the so-called mental diseases only embrace a certain moderate proportion." In the enunciation of this axiom, a great step was gained; and had its author been spared to work out his theory, we might by this time have had a definite system before us.

Maudsley's classification is an elaboration of the old psychical system; it will never stand.

Van der Kolk has begun at the beginning, and has stopped there. But he has made his mark in commencing a system. He divides so-called mental disorders into two great divisions—viz., the idiopathic, those that originate within the brain itself; and the sympathetic, those which are caused by disturbance in the general system, acting secondarily on the brain. This much he has done, and, in so doing, has only suggested a scheme which remains for future thinkers to elaborate. He is content with two great classes, which he says "serve all considerations in a therapeutical point of view." Here we cannot follow him; for, if under idiopathic insanity he classes idiocy, traumatic insanity, epilepsy, general paralysis, and congenital insanity; and if under sympathetic he groups insanity resulting from masturbation, childbirth, intestinal irritation, or alcohol, it is difficult to conceive how the two classes can serve as guides for therapeutical treatment, when we reflect how widely separated is the etiology of the constituent elements. Nor do we think that this author has gone far enough in the establishment of grand classes. There are certain diseases, such as epilepsy and general paralysis, which arise idiopathically and sympathetically in different cases, but which, whatever their origin, must be accepted as distinct pathological entities, and classed by themselves. What we now want is, that the theories of Skae, Griesinger, and Van der Kolk should be amalgamated; that the clinical differentiation of the first, the broad generalization of the second, and the physiolo deductions of the third, should be made to act one on the ot some mind which can expiscate the truth presented to it from these three points of view.

It cannot be said that the latter half of this book is of any value to the student. The opinions of its author are of great interest to the physician, whose experience will enable him to gather many valuable hints from accurately recorded cases; but with the exception of the one point we have noted, no such importance attaches to the latter part as is contained in the former.

That the translator's work has been done with a very servile adherence to the idiom of the language of the original, is easily seen by reference to the passage quoted above, also by such sentences as these:—"The hereupon following corpora quadrigemina are very large." "Still further forward, at the root of the olfactory nerve, must then the perception of smell take place." The revision of proof must also have been very imperfect, when we meet with "premisses" twice in one page, and the ridiculous misprint of "*flores amicæ*" for *flores arnicæ* left uncorrected.

We are still in want of a text-book on mental diseases; for all practical purposes the work of Tuke and Bucknill is the best extant, but even it is behind the time.

## RECENT WORKS ON DISEASES OF THE EYE.

(SECOND NOTICE.)

1. *Treatise on Diseases of the Eye, including the Anatomy of the Organ.* By CARL STELLWAG VON CARION, M.D., etc. Translated from the third German edition, and edited by CHARLES E. HACKLEY, M.D., etc., and D. B. ST JOHN ROOSA, M.D., etc.; with an Appendix by the Editors. London: Robert Hardwicke: 1868. Pp. 774.
2. *A Treatise on Diseases of the Eye.* By J. SOELBERG WELLS, Professor of Ophthalmology in King's College, London, etc. London: John Churchill & Sons: 1869. Pp. 741.
3. *A Manual of the Diseases of the Eye.* By C. MACNAMARA, Surgeon to the Calcutta Ophthalmic Hospital, etc. London: John Churchill & Sons: 1868. Pp. 570.

THE first work on our list is in all respects a typically German treatise, being a most elaborate compendium of what has been written by German authors on diseases of the eye, and also embracing most of the important facts which have been added by workers in other countries. Most subjects are treated of at great length, the minutest points being carefully detailed, and, as almost necessarily occurs under such circumstances, there is a considerable amount of repetition. What, however, we have most to complain of, is an utter want of conciseness and clearness in our author's descriptions. Instead of proceeding at once to the essential point, he indulges in ever so much wordy generalization, which, from its very long-windedness, and often obscurity, serves rather to mystify than to render what follows the least degree clearer. To these disadvantages must be added those which unavoidably attend a translation, the author's meaning, in difficult or obscure passages, being seldom rendered more distinct by being subjected to a translator's analysis. The arrangement of the work, too, is capable of much improvement; diseases closely allied to one another, and often dependent upon each other, being considered under different sections. Although the views of most authors on the various subjects are given (and excellent tables of references appended to each chapter), Von Carion indicates, in most cases, very decidedly what his own opinions are. In the preface, he states that he has avoided any allusion to his own writings in the text, which we found to be the case, but which we cannot help considering a subject of regret, as detracting from the completeness of the work. When, however, he goes on to remark, that those who are familiar with the literature of ophthalmology will find many deviations when the citations of this work are compared with those of some other modern writings, and attributes this to having kept himself "strictly within the bounds of historical truth, and disdained to injure or advance the interests of any persons or schools by any misplacement of authorities," we feel bound to point out that this is not in all respects correct; for while, as regards Continental authorities, his references may be perfectly accurate, he throughout his work very much overlooks what has been written and done in this country. Thus, as regards the Calabar bean, while, under the list of authorities, the papers of Professor Christison and Drs T. R. Fraser, Argyll Robertson, and G. Harley, and Messrs Bowman and Soelberg Wells, find a place in the text, Graefe is given as almost the only authority for the effects produced by it on the eye, while in reality they were, with few exceptions, first detailed by Drs Fraser and Robertson. On anæmic exophthalmos, too, we do not find the slightest reference to Graves or Begbie, both of whom were among the very earliest (if not the earliest) to describe the symptoms of this compound disease. These illustrations may suffice, but we may here further refer to some omissions of importance, which we incidentally noticed, and which lead us to question the completeness of his vaunted information. We have looked in vain for the slightest reference to Desmarres's operation for pterygium, Teale's operation for symblepharon, the use of a seton for the cure of trichiasis, Stokes's method of treating granular conjunctiva by a compressor; and while, in describing operations on the eyelids, frequent reference is made to the troublesome hæmorrhage that accompanies them, all allusion to Snellen's forceps, whereby this may be effectually prevented, is omitted.

We have considered it necessary to point out these defects, in t hope that in any future edition they may be remedied, and English ophthalmic literature, in particular, be more recognised. We, however, most heartily acknowledge that Von Carion's work will always be a standard one in ophthalmology, and that it is one of the most useful works of reference that an oculist can possess. On many points we are inclined to differ from the author, in particular as regards the non-existence of a dilatator pupillæ muscle, the inutility of the Calabar bean in paralysis of the accommodation and in mydriasis, in the causation of ophthalmia neonatorum by bright light, and the too limited commendation of nitrate of silver as a local application in purulent ophthalmia.

Thanks are also due to the painstaking American surgeons who have translated this extensive work. It is said that the late Emperor of Russia forbade, during the Crimean war, the teaching of the English language in his dominions, and directed that the American language should be taught instead. We have hitherto been at a loss to understand his distinction; but since reading this translation our difficulties have disappeared, for it certainly is not English in spelling, and is often doubtfully English in grammar; but we suppose it is good American.

We have had much pleasure in studying the work of Mr Soelberg Wells, embodying, as it does, all the most recent investigations, both home and foreign, and in particular because the author appreciates and expounds, on most points, the views of Professor Von Graefe, which on further investigation have, in almost every instance, proved to be correct. In most of his descriptions, Mr Wells is clear and precise, and above all, as regards treatment, gives distinct directions as to the means which have proved most beneficial in his own hands. In this respect the work before us is of much more service to the general practitioner than those heavy compilations which, in giving every person's views, too often neglect to specify those which are most in accordance with the author's opinions or in general acceptance. When he does quote the opinions of others, we would recommend references to be more frequently given; for while we have never found him fail to make the author's meaning distinct, we have repeatedly wished to make further acquaintance with the papers from which he quotes.

Mr Wells has limited himself entirely to a consideration of the diseases to which the eye and its appendages are liable, without sketching the anatomy and physiology of the different parts, as is the case in many treatises on ophthalmic surgery. To the majority of readers this is no loss, as information on these points is not generally much sought after by the busy practitioner; while, for a clear comprehension of most of the diseased conditions of the eye, a moderate general knowledge of the anatomy and physiology of the organ suffices. We must, however, except from this statement, paralytic and other affections of the muscles and abnormal conditions of the refraction and accommodation of the eye, and we are glad to observe that on these very points Mr Wells has departed from his general rule, and prefixed a short anatomical and physiological resumé. In his description of ophthalmoscopic affections, Mr Wells enters pretty fully into the microscopic appearances presented by the diseased tissues and the pathological views of Continental authors. This, we think, might, with advantage (especially in the case of the different forms of retinitis), be considerably curtailed. Having thus expressed our views regarding the general character of the book, let us notice a few points which we have marked for special consideration.

The introductory chapter on the modes of examining the eye, etc., is well worthy of attentive perusal, as most of the points there alluded to are of great importance, and some of them much neglected in general practice. We would particularly direct attention to the mode of ascertaining the acuteness of vision and the extent of the field of vision, and his explanation of the different varieties of double images.

The chapters on diseases of the external coats of the eye are in all respects good and trustworthy, and the treatment recommended is in accordance with the most approved modern doctrines. We would, however, enter our protest against the use of a syringe for cleansing the eye affected with purulent ophthalmia. Most authors apparently approve of this proceeding; but it is self-evident that it is attended with great danger at once to the patient, to the surgeon, and to the nurse. To the patient, because if the cornea be ulcerated, as is very often the case, perforation may be induced by the pressure of the nozzle of the syringe against the eye; to the surgeon and nurse, because in syringing out the contagious matter a little may be propelled into their eyes. In all cases of this disease, the eyes may be safely and efficiently cleaned by the use of small pieces of lint, soft rag, or cotton wool.

The chapter on chronic granulations is particularly good; but Mr Wells errs in attributing the employment of inoculation for the cure of pannus to Piringer, as in this he was preceded by Dr Henry Walker, who recommended it in 1811.

While allowing that inherited syphilis is *one* cause of diffuse corneitis, as has been most ably pointed out by Mr Jonathan Hutchinson, Mr Wells, very correctly we think, points out that this form of keratitis may occur in persons quite free from syphilitic taint, and who present none of the characteristic signs of inherited specific disease. He, therefore, objects to the term "syphilitic corneitis;" but here we disagree, for just as one variety of iritis is termed syphilitic, because, although it cannot be discriminated with certainty by the character of the inflammation from other forms, it still is certainly dependent upon the syphilitic poison; so in like manner we may here adopt the term syphilitic keratitis (or corneitis) to indicate that in many cases the disease is dependent upon or connected with inherited syphilitic taint.

The different operations for cataract which are in general use at the present time are clearly described, and the indications for the employment of each pointed out. Mr Wells is greatly in favour of Von Graefe's modified linear extraction in cases of hard cataract, and condemns reclination (which he very shortly describes) *in toto.* We think that reclination may still be judiciously performed in a few exceptional cases, where the patient is *very* old and feeble, and extraction specially contraindicated, and therefore is deserving of a place among the recognised operations for cataract. Mr Wells speaks more favourably of the operation by suction than we are inclined to do. We are decidedly of the opinion that the fewer instruments that are introduced into the eye for the removal of cataract, the better the result; and when we have so satisfactory an operation, though slow, as division for soft cataract in young patients, we consider it a temptation of Providence to employ a suction curette simply with the view of effecting a more rapid cure.

Mr Wells devotes a space corresponding to their importance to those deep-seated affections for the recognition of which the use of the ophthalmoscope is imperative. The chapter on amblyopia is, in particular, worthy of commendation. Under affections of the choroid, tubercular deposits are carefully described, and a case narrated which came under the author's own observation. We are of opinion that, in doubtful cases of acute tuberculosis, an ophthalmoscopic examination may often determine the nature of the disease; and it should not be forgotten that to Mr Wells we are indebted for having directed the attention of the profession in this country to this important subject. In cases where local bloodletting is indicated, the use of Heurteloupe's artificial leech is recommended.

The profession have long been familiar with Mr Wells's account of Von Graefe's elaborate investigations into the normal action and diseased conditions of the muscles of the eye. These he has reproduced in a condensed form in the treatise before us. We noticed, however, one slip which requires correction. In describing the double images which occur when the inferior rectus is paralyzed, he states that the pseudo-image lies *above* the image seen by the healthy eye, whereas the opposite is the case. We can also speak favourably of his description of the anomalies of refraction and accommodation, which gives shortly the views which Donders has so fully expounded in his standard work.

The text is illustrated by numerous woodcuts, and, with Liebreich's sanction, several ophthalmoscopic plates are excellently reproduced from his magnificent atlas.

We have no hesitation in recommending this treatise, as, on the whole, of all English works on the subject, the one best adapted to the wants of the general practitioner.

Mr Macnamara's work is one of Churchill's manuals, and is stated in the preface to contain almost *verbatim* the course of lectures he delivered to the students of the Medical College, Calcutta, during the session of 1867. Mr Macnamara's experience is mainly derived from a study of ophthalmic affections in the persons of natives of India, and the chief value of his work, in our opinion, consists in his description of several affections almost peculiar to a warm agueish climate, and of others which are modified by such surroundings.

We can speak with unqualified approbation of the arrangement of the book, and specially commend the use of marginal headings, whereby we can with great facility find information upon any particular point. We only wish voluminous authors, more especially German, would follow this excellent example: what a weariness of the mind would it not often save their unfortunate readers!

Mr Macnamara also deserves praise for his description and representations of the appearance presented on ophthalmoscopic examination of the eyes of the natives of India, both healthy and pathological. The recent views of home and Continental ophthalmologists are by no means overlooked; and as regards treatment, the author condemns unhesitatingly the antiphlogistic measures formerly so much employed in inflammatory affections, and appears to harbour a particular aversion to calomel and leeches. This manual must not be looked upon merely as a text-book in which an outline of the generally adopted views of the different affections is given; for, on some subjects, peculiar doctrines are advocated, quite opposed to those maintained by most oculists of the present day. In glancing through the work we noted one or two points for comment. In his short preliminary chapter on the anatomy of the eye, he mentions that the lens is more convex in front than behind, and is composed of a numerous series of fibres of a peculiar form, and probably of a contractile nature. It is well known that the anterior surface of the lens is *less* convex than the posterior; and the theory that the lens is muscular in structure, although propounded a century and a half ago, and maintained by John Hunter, Young, and others, has remained unsupported by any good proof; while the fact that the application of electricity to a lens immediately after removal from a living eye does not induce any alteration in form, suffices, in our opinion, to overturn such a view. Mr Macnamara, however, has made recent investigations into the minute anatomy of the primitive muscular fibre, and also of the fibres of the lens, and from the structural analogy which appeared to exist between them, convinced himself of the muscular nature of the latter. He also maintains that he has traced nerves over the capsule of the lens. In his investigations he placed the lens and capsule after removal in glycerine for a month, and a suitable portion being then carefully detached, was examined under a one-fiftieth or one-seventieth of an inch object-glass. Examinations conducted under such circumstances are necessarily open to numer fallacies. Holding these views, Mr Macnamara naturally ascribes the act of accommodation, not to the ciliary muscle, but to a contraction in the lens itself. In his description of the disorders of the accommodation, however, we were amused to observe that he assumes "that the ciliary muscle is the active agent in altering the curvature of the lens, *because this is the commonly received doctrine on the subject.*"

In treating of the "examination of the eye," Jaeger's test-types are stated to be those commonly employed, and a series of test-types corresponding to Jaeger's are appended; but in the chapter on disorders of accommodation and refraction, Snellen's types are described, and are those invariably referred to. We trust that in the second edition Jaeger's types may be replaced by Snellen's, which are unquestionably the better. For ophthalmoscopic examination, the author prefers sunlight to that of a lamp, an assistant being sent out "into the sun" with a mirror, from which the rays of light can be reflected through an open door or window into the room. We have had no experience of this method, which is certainly not suited to this climate, and, in consideration for his patient's eyes, we are glad to learn that his examinations are not prolonged. Our author is fond of sunlight, and mentions that it may be employed for oblique illumination. We warn our readers against subjecting the delicate cornea to the concentrated rays of the sun—a proceeding which would at once startle their patient and reflect little credit on the surgeon's judgment.

The descriptions of purulent and granular conjunctivitis are very good, and the treatment recommended, in our opinion, judicious; we would, however, suggest that it is not by any means necessary, except in the case of extreme restlessness and irritability on the part of our patient, to have recourse to chloroform for the purpose of examination, which is by him recommended to be invariably done. It must be borne in mind that sickness and vomiting not unfrequently occur after its administration, and that this would subject an eye with extensive ulceration of the cornea to more danger than a careful examination without chloroform. The doses of opium recommended are much larger than we are accustomed to order in this country. In granular conjunctivitis it is recommended that, if the patient be in pain, a grain of opium should be given three times a day, and should the disease gain ground, we must increase the quantity of opium to *two grains three times a day.*

In the treatment of iritis, after mentioning how mercury may be administered, the following footnote is appended:—"It will probably be asked upon what principles I advance the above system of treatment? I can only answer that I am not responsible for it; it is the treatment advocated by almost all the leading surgeons of the day, and I think I am bound therefore to promulgate it. In my own practice I seldom, if ever, give mercury in instances of iritis, believing that cases which recover while it is being given would get equally well without it, and that those doing badly would do so in spite of it. After the most careful consideration, I hold that mercury is neither antiplastic nor antisyphilitic, *but that in certain secondary and hereditary cases of syphilis it exercises a favourable influence on the diseased tissues.*" The last portion of the note entirely invalidates what precedes it. No one at the present day, it is to be hoped, would think of giving mercury in all cases of iritis indiscriminately, while few who have had much experience in the treatment of eye diseases will deny the benefit to be derived from mercury in many instances. From the preface of the Manual, however, we learn that the author thinks he has not found it necessary to use a leech, or to give a grain of calomel, to his patients in the Calcutta Ophthalmic Hospital during the last year, although he had a daily average of some fifty indoor patients.

Among ophthalmoscopic affections we find described what the author terms melanæmia of the retina—a condition presenting very much the appearance of retinitis pigmentosa, but occurring during intermittent fever, and attended by, or rather dependent upon, the presence of small masses of pigment in the blood. As this is, as far as we are aware, a hitherto undescribed affection, we extract the case which is cited as an example:—

"Biddoo, a labourer by occupation, aged thirty-five, applied for relief at the Ophthalmic Hospital on the 19th May 1864. He is a well-built, strong-looking man, rather anæmic, but states that, with the exception of attacks of fever, he has always enjoyed good health. About six months previously, being in a very malarious part of the country, he was seized with remittent fever; prior to this his eyesight was perfectly good. He underwent great privations, and six days after the fever came on he found his sight growing dim; he had no pain in the eyes, nor was there any photophobia, but from that time till his admission the field of vision gradually contracted, and he could then only just see to grope his way about the room.

"I found the tension of both eyeballs natural, the pupils dilated, but there was no external indication of disease. On examining the patient's eyes with the ophthalmoscope, I discovered the optic discs of both to be of normal size and colour. The vessels of the retina were much contracted, and at the outer part of it a number of elongated black spots were noticed. Some of the black lines appeared to be the smaller branches of the arteries, which had been plugged with pigmentary matter. In many places, however, it was impossible to trace the atrophied vessels up to the point at which they had become occluded, whereas, in other branches, small deposits of pigment could be detected actually filling up the vessels.

"Besides these pigmentary deposits, there were a number of faint dark markings observed in the substance of the retina itself, which seemed to me to arise from a stained condition of its nervous tissue, consequent on the previous circulation of pigmentary matter in the capillaries, the larger masses having been subsequently stopped in the smallest branches of the arteries. The fundus of the eye was of a dusky-red colour in this case, and there were no hæmorrhagic spots to be seen on its surface.

"The patient had not been in the hospital long before other cases of a precisely similar nature presented themselves for treatment; not that these are a common class of cases, but, as so often happens in hospital practice, a certain form of disease appears at times to have a run, and then to disappear for weeks and months together. However, these cases coming one after anoth and each of them having a similar story to tell, of blindness superveni repeated attacks of remittent fever, led me to think that the changes noticed in the eye must have arisen from the same cause, and one closely connected, in some way or other, with malarial influences.

"Shortly after Biddoo came under my care he was attacked with fever, and during a remission on the 9th of June, I opened a vein in his arm, and drew off a small quantity of blood; a portion of it was at once placed under the microscope, and I quote the appearances it presented in the words of the Professor of Chemistry. He says, 'Four or five or even more pigment cells or masses could always be seen at one time in the field of the microscope. The cells generally resembled the white corpuscles of the blood, but contained many black granules. Some of these cells were considerably larger, and of an oval shape. Together with the cells, irregularly-shaped masses of pigmentary matter were to be seen in the field of the microscope.' If we compare this description with that given by Frerichs, and with his representation of pigmentary matter from the portal vein, we shall find them to be very much alike."

In extraction of cataract, Mr Macnamara recommends the administration of chloroform. With restless patients the operation may thus no doubt be often more satisfactorily performed, but the advantage so obtained is more than counterbalanced by the obstinate sickness which not unfrequently follows. It is only in the most exceptional cases that we would think of employing it. Iridectomy, which most surgeons now consider of such advantage, both in facilitating the exit of the lens, and diminishing the subsequent risk of inflammation, is viewed unfavourably by our author. He generally operates by scoop-extraction, without the removal of a portion of iris.

We regret that we cannot recommend this Manual as in all respects a safe or trustworthy guide, but at the same time are happy to admit that it contains much information that may prove serviceable to surgeons in India. We hope that a second improved edition may soon appear.

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*The Essentials of Bandaging, including the Management of Fractures and Dislocations, with Directions for using other Surgical Apparatus.* Illustrated by 122 Engravings on Wood. By BERKELEY HILL, M.B. Lond., F.R.C.S., Instructor in Bandaging in University College; Assistant-Surgeon to University College Hospital; and Surgeon to Out-Patients at the Lock Hospital. Second Edition, revised and enlarged. London: James Walton: 1869.

WE have much pleasure in noticing the second edition of this excellent little Manual, the first edition of which was pretty fully criticised in these pages on its appearance upwards of two years ago. We have now very little to add to our previous criticisms. The bulk of the work remains unchanged. Here and there we notice additions to the text. One very useful method of rapidly and simply applying a plaster protection to fractures, is that devised by Mr Moffitt, and described in the Army Medical Reports for 1865, and Mr Hill has wisely, we think, inserted a pretty full account of it and a woodcut (pp. 88, 89). In describing the modes of reduction of dislocations of the hip, we observe that Mr Hill has made no change in his remarks in the first edition regarding the manipulation methods, which then and now he seems to regard with less favour than some other surgeons do. The chapter on Catheters and Bougies is much enlarged and improved. It now includes an account of the method (first, we believe, introduceed by Dr Gouley) of treating a very narrow stricture by the use of numerous filiform bougies. There is one evidence of careless correction of proofs—a sentence repeated *verbatim* (in pp. 130 and 135), which may be corrected in a third edition.

A figure and description of Dr Horace Swete's cheap ambulance is given in this edition. It seems a very simple, useful, and cheap machine. Lister's method of dressing wounds with carbolic acid, and the details given, are made out up to the date of publication. More recent changes in the method may have made some of the description already antiquated, but the principle seems thoroughly understood.

The administration of chloroform is described and still illustrated by the hideously-misleading picture of the operator with his bagpipe giving chloroform to an old gentleman, who is sitting bolt upright, while the operator feels his pulse with an expression of great interest. This is followed by a full account of the methods of carrying on artificial respiration called by the names of Marshall Hall and Sylvester; the old gentleman, however, is not the sufferer, as he might have been expected to be.

An appendix of 16 pages is added to this edition, giving lists of the various instruments and appliances needed by the surgeon for most of the more important and ordinary operations of surgery. These may be useful to some surgeons, but are rather carelessly got up, being sometimes redundant and sometimes deficient. For example, it might be supposed that, as a general rule for most operations of surgery, sponges, water, and towels are needed, and chloroform may almost be presupposed as probably necessary. These essentials are repeated in nearly every one. Indeed, for Amussat's operation, p. 205, we require two sets of sponges; while for Cæsarean section sponges are unnecessary, and we must be content with either flannel or cotton-wool. It is difficult also to understand how such a list can be supposed to be a suitable addition to a work entitled "The Essentials of Bandaging."

# Part Third.

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## MEETINGS OF SOCIETIES.

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## MEDICO-CHIRURGICAL SOCIETY OF EDINBURGH.

### SESSION XLIX.—MEETING IV.

*Wednesday, 5th January* 1870.—Dr BENNETT, *President*, in the Chair:

I. *Mr Annandale* introduced a gentleman, who in 1869 had suffered from CARIES involving a finger-joint, for which excision had been practised with an excellent result.

II. A TIN KNEE-SPLINT, with an ingenious spring adjustment, by which the joint could be flexed or extended at pleasure.

III. *Dr A. G. Miller* showed THE ARM of a woman which he had removed on account of a severe railway accident. The wheel of an engine had almost entirely severed the arm from the body.

IV. *Dr Joseph Bell* exhibited A FATTY TUMOUR removed by himself from the forehead of a patient; the situation being a rare one for tumours of that character.

V. *Mr Annandale* then read his communication upon FATTY HERNIA, which is published at p. 769 of our present number.

*Dr Watson* stated that the subject did not seem to him to be deserving of special elucidation, as the amount of extra-peritoneal fat in cases of hernia was well known to be very variable.

*Dr Joseph Bell* considered the chief interest of the paper to lie in the narration of examples of protrusions of fat in places which were not ordinarily the seats of hernia.

*Mr Annandale* made a few remarks in reply, and insisted that, so far as the records of surgery were concerned, the facts he had described were new, and, as he believed, important.

VI. *Dr Watson* next related a case of POPLITEAL ANEURISM treated by compression. The patient and the apparatus employed were exhibited.

This case will appear in an early number.

*Dr Joseph Bell* thought that compression was of great service in enabling the surgeon to direct his attention now to one part of the arterial course, and then to another if necessary.

*Dr John Duncan* referred to what was often termed the rapid solution of the aneurismal clot. What he imagined actually took place in such cases was, that the thin peripheral layer of fibrine was broken through by one of the afferent arteries.

*Dr Argyll Robertson*, referring to the employment of the iodide of potassium in this case, asked Dr Watson if he had ever observed any curative effects from iodide of potassium in cases of aneurism.

*Dr Watson*, in reply, explained his views as to the changes that occurred in aneurismal sac undergoing consolidation. He ascribed the benefit derived from iodide of potassium in such cases chiefly to its soothing effects. He also threw out the suggestion that the arterial disease might possibly, in some cases, be of syphilitic origin, and that the iodide might act beneficially on that account.

*The President* pointed out the difficulty of distinguishing the effects of iodide of potassium from those of the other treatment simultaneously employed—such as rest, regulated diet, etc.

*Dr Joseph Bell* referred to a case of internal aneurism in which iodide of potassium was employed with such great relief to all the symptoms that the patient always demanded its re-employment when from any cause it was temporarily discontinued.

*Dr Watson* alluded to Grassi's observations, which seemed to show that, in cases of anæmia from syphilis or mercurio-syphilis, iodide of potassium increased the number of the blood-corpuscles.

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## SESSION XLIX.—MEETING V.

*Wednesday, 2d February* 1870.—Dr BENNETT, *President*, in the Chair.

I. *Dr Argyll Robertson* exhibited a man affected with a PECULIAR AFFECTION OF THE CONJUNCTIVA of the right upper eyelid. The patient complained of irritation and stiffness of the affected lid. On examining the eye, Dr Robertson found the cornea very slightly opaque and vascular at its upper margin; and, on everting the lid, noticed the conjunctival surface covered with minute elevations, which did not yield to the application of nitrate of silver, or other stimulant or astringent application. Not having seen such a case before, he removed some of the elevations, and examined them under the microscope, and found them to consist of epithelial cells, containing calcareous matter in a molecular form. These productions had been twice removed, but, after a lapse of several weeks, became re-formed. They were always quite superficially seated, and could be removed without inducing any bleeding. As far as Dr Robertson was aware, no such case has been previously recorded.

II. *Dr P. H. Watson* showed AN ENTIRE TONGUE, which he had removed for epithelial cancer of the organ. The disease had originated as a nodule upon the right side, and had been removed by partial excision in the country. The disease had, however, recurred more deeply in the organ. The operation performed was by the division of the lip and symphysis of the jaw. The mylo-hyoid and genio-hyoid muscles were left intact.

III. AN ENTIRE SCAPULA and the outer third of the clavicle, removed on account of medullary cancer. The case was one in which

Dr W. had previously removed a portion of the scapula, and an account of which he had read to the Society, when one of the speakers expressed his opinion, that the affection was not of a malignant nature, but simply a soft, rapidly-growing cartilaginous tumour. The return of the tumour, its rapid growth, its extension posteriorly in the texture of the rhomboids, sufficiently attested the essentially cancerous nature of the tumour, apart from its microscopic characters.

IV. A cast and the dissected specimen of A VERY LARGE TUMOUR OF THE FEMUR, on account of which he had performed amputation at the hip-joint. The patient had suffered from the disease for two years. A year ago the attenuated bone had given way in walking, and since that time the disease had made rapid progress. The structure of the tumour was cystic, and, from microscopic examination, appeared simple in its nature; a view borne out by the painless character of its growth, and the absence of all glandular complication.

V. A FOOT, removed to-day by the ankle-joint amputation, on account of disease of the tarsus and its articulations, especially involving the ankle-joint, tibia, astragalus, and os calcis. The disease, from its history, had commenced in the synovial membrane, involving the osseous tissues only secondarily. The extension of the disease was very great; so much so, that a second slice of the tibia needed to be cut away. The vessels were in a softened, spongy condition, and could scarcely hold a ligature. The little lad had suffered for more than two years, but had latterly become more exhausted and emaciated. He had albuminuria, probably due to amyloid degeneration of the kidney. As the discharge from the foot underwent no diminution under the influence of treatment, and as the kidney degeneration was quite as likely to be caused and maintained by the suppurative waste as to be the occasion of its continuance, Dr Watson deemed further delay inadmissible.

VI. *Dr Joseph Bell* showed A TUMOUR OF THE MAMMA, which he had removed from a lady aged 76. The tumour had been noticed for only three weeks, and was increasing with great rapidity. On section, the tumour showed several cysts, which seemed to contain simply coagulated blood. The posterior part of the tumour was continuous with the breast tissue, which it infiltrated. The patient made a good recovery.

VII. *Dr Bennett* showed a specimen of CANCER OF THE PERICARDIUM, received from Dr Robert Clay of Plymouth.

The history of this case is published at p. 802 of our present number.

VIII. *Dr Bennett* also exhibited a modification of Marey's SPHYGMOGRAPH devised by Messrs Mayer & Meltzer of London.

It differed from its prototype in being remarkably easily put on and adjusted, and also in possessing the power of regulating the pressure applied to the artery, without which comparative experiments on the same patient could not be carried on, and the instrument was thus deprived of one half of its utility.

IX. *Dr Robertson* next read his paper entitled A CASE OF DIPHTHERITIC CONJUNCTIVITIS, which is published at p. 781 of our present number.

*Dr Bennett* remarked that the diagnosis of a disease should not be too strongly based upon the fact of its being epidemic or sporadic. He stated that it was an undoubted fact, the truth of which many besides himself must have recognised, that many diseases, usually regarded as epidemic, occasionally occurred in a sporadic and isolated form. Some time ago, when living with his family at Arrochar, he had been requested to see the infant child of the innkeeper there, aged ten months; on opening its mouth, he found it covered with the thick greenish crust of diphtherite. The old woman, on whose knee the infant lay, who was also the wise woman of the village, said she had never seen such a case in all her experience, and had never heard of such a disease as diphtheria. As the innkeeper had some six or eight other children, the question of isolation had to be considered, and in the view of the many difficulties attending such a step, it was resolved to say nothing about it. The child died, the disease did not spread, and this was the sole case of diphtheria that occurred in the district. He could give many other examples of epidemic diseases occasionally assuming the sporadic form, amongst the rest of cholera, and such cases had unquestionably a most important bearing on the causation of such diseases. There was no reason why an isolated case of diphtherite should not occur independently of an epidemic.

*Dr Joseph Bell* remarked on the extreme rarity of the occurrence of such cases without the subsequent appearance, within about two months, of some paralytic sequelæ. Of these, the most frequent was an affection of the fourth nerve, resulting in diplopia, which might disappear in a few days; the next most frequent being a paralysis of the velum, which was pendulous and immobile, and produced a nasal twang in the speech. He considered that it was a most important point in the diagnosis of such cases to ascertain the occurrence of any such sequelæ, or their absence. He also remarked upon Von Graefe's treatment, and its remarkable success, as being somewhat inconsistent with our views and experience of diphtheria.

*Dr Robertson* stated that no epidemics of this disease had ever been recorded as having occurred in this country, but that many such had happened on the Continent, especially in Berlin, few fatal cases having been recorded. Of course, where the throat was chiefly affected, as in our epidemics, the mortality was naturally mu increased, first by the disease passing into the larynx, and, second, by the continuous poisoning of the system by the matter swallowed, and the larger surface affected. In the comparatively trifling affection of conjunctival diphtherite, such accidents, of course, could not, and did not, occur.

*Dr Watson* remarked on the extreme interest of the case, though he doubted its being a *bona fide* case of diphtheritic conjunctivitis, especially as in such a case he should certainly have expected some paralytic sequelæ. He suggested that the reason why the mucous membrane of the eye was less often affected than that of the throat, was that the constant nictitation might remove the poisonous germs. He also remarked upon the occurrence of similar non-diphtheritic false membranes in the eye resembling those found beneath the foreskin in cases of gonorrhœa, and suggested that this might be a similar case affecting the eye. He also remarked on the somewhat astonishing nature and results of Von Graefe's treatment, and suggested that all the cases thus treated could not be regarded as the result of the depravation of the blood by a depressing poison, which diphtherite essentially was.

*Dr Robertson* remarked, that in one essential point his case had differed from those cases of gonorrhœal ophthalmia to which Dr Watson had alluded, viz., that in it the membrane when picked off by the forceps was not found loose, but on its detachment left bleeding points behind it, and that also the infiltration of the lids was firm and cartilaginous, and not simply serous.

*Dr Bennett* remarked, *apropos* of germs, that it was a wonder to him we were ever well in any part of our frame, considering the number of these death-dealing germs continually floating around us, as described by Professor Tyndall. Tyndall's germs were nothing but dust, and dust was well known to the histologist to consist of starch-corpuscles, fragments of cotton, wool, and silk, and fragments of mineral and vegetable substances. The germs giving rise to bacteriæ and vibriones were much more minute and essentially different in character. Pasteur's germs were simply starch-corpuscles.

X. *Dr Watson* then read his case of SLOUGHING HERNIA, which is published at p. 786 of this Journal.

*Dr Bennett* remarked, that if putridity and fermentation depended upon the penetration of germs into the closed sac,—how these penetrated had, at all events, never yet been explained. The same difficulty occurred as shown by Donné in the case of a rotten egg. He called attention to the difficulty of treating cases of obstructed bowels in which the position of the obstruction was unascertainable, and thought it good practice to attempt to overcome the obstruction by the administration of one good purge. If this experiment failed, it should not be repeated, and other measures should be resorted to; but he thought the attempt ought first to be made, and this threw a good deal of responsibility upon the practitioner.

*Dr Gillespie* stated that Dr Watson had referred chiefly to the after-treatment of such cases, in which formerly surgeons used to give purgatives freely with the view of relieving the over-distended intestine. He quite concurred in the view that a piece of bowel which had been strangulated and inevitably handled required rest, and in such circumstances he considered that the postponement of a purgative for a week or more constituted one of the most important points of treatment. In treatises on such diseases it was usual to recommend the laying open of the bowel when it was found to be sloughing; but inasmuch as all the coats were not always found thus affected, but occasionally only the peritoneal one, as in this case, and as a fæcal fistula involved a short life, he considered this case a most important one.

*Dr J. Bell* said that he considered the case to have been remarkably well managed; that, notwithstanding a difference of opinion in the older authors, more recent works gave forth no uncertain sound, the rule laid down being to administer opium freely if required, and never to purge—to wait patiently for three or four weeks, or longer, till the bowels acted of their own accord.

*Dr Watson*, in reply, stated that the old doctrine of giving one good purge primarily in all cases of obstruction indiscriminately he considered a most dangerous one, and often attended by deplorable results, the purge being sometimes found in the cavity of the peritoneum instead of in the close-stool. He thought that a better rule in cases of internal obstruction was to avoid purgatives, as the bowels naturally tended to relieve themselves in time if the case were one of simple obstipation; while if pain was the prominent symptom, the proper rule was at all hazards to attempt to soothe the pain in the first instance.

*Dr Bennett* remarked, that however it might be in surgery, where the rules for practice were usually more definite, he was quite sure that in physic, especially in hospital practice, the best rule of treatment for cases of obstipation was to commence with one good purgative. He had seen many cases in which the obstruction was thus overcome, and had never observed any deplorable results to arise from such a procedure. Besides, whenever no such effort had been made, it was impossible to tell whether the obstruction would or would not yield to a purgative.

*Dr Strachan of Dollar* remarked, that in country practice the patients had already tried the purgative treatment sufficiently before the doctor was called in.

*Dr J. Duncan* stated, in regard to the subsidence of pain on the occurrence of suppuration, that he had seen the same thing occur in one of Professor Inglis's (now of Aberdeen) patients. In this case the pain and sickness subsided in three or four days, but as there was a persistent tumour in the groin the operation was performed. Three-fourths of the surface of the bowel was found sloughing, and a fæcal fistula resulted, which was cured by a wire ligatur

## OBSTETRICAL SOCIETY OF EDINBURGH.

### SESSION XXIX.—MEETING III.

8*th December* 1869.—Dr BELL, *President*, in the Chair.

Alexander M'Farlane, M.D., C.M., and Isaac Mossop, L.R.C.P. & S., were admitted as Ordinary Fellows of the Society.

I. *Dr James Young* exhibited a CHLOROFORM INHALER, which had been invented by Dr Ellis of London.

*Dr Pattison* objected to the instrument on account of its expense.

*Dr Cairns* thought the instrument would be difficult to manage, and, in tossing about, the patient might smash it.

*Dr Keiller* said, that long ago, Sir James Simpson had endeavoured to get an instrument constructed for the inhalation of ether, and at that time a bottle with a mouth-piece was tried. But latterly all instruments were discarded, and he thought there was no appliance so good as the simple towel or handkerchief.

*Dr Frazer* thought that this instrument would do well for dentists.

*Dr Lambert* said that in France the surgeons now preferred the simplest appliances.

*Dr Bell* thought this instrument too complicated. There had been very few fatal cases from chloroform in Scotland as compared with England, and this he thought was owing to the mode of administration.

*Dr Dickson* said he found it a good plan to roll a towel into the form of a tube, and in this way the chloroform was properly mixed with air.

### II. A CASE OF PUERPERAL CONVULSIONS. BY DR ANGUS MACDONALD.

The discussion on this paper was resumed by *Dr Pattison*, who wanted to know if Dr Macdonald had given the bromide of potass in the case he had related. He thought it would have had good effect. He had seen its good effects in *delirium tremens*. In convulsions he had lost one case in four.

*Dr Cochrane* had also seen the bromide have good effects in *delirium tremens*.

*Dr Macdonald* replied, he did not think the bromide of potass would have been of use whilst the uterine irritation existed.

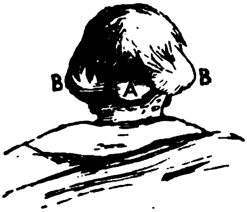
*Dr Young* stated he was called to a case in which, when he saw the patient, the os was the size of a shilling-piece. From 11 P.M. to 6 A.M. convulsions came on every half-hour, and continued after delivery. The patient was leeched and blistered. The urine was also found to be albuminous. The patient died the same night. He questioned whether general bleeding would have been useful. There was no unusual amount of discharge.

## III. CASE OF A CHILD BORN WITHOUT AN OCCIPITAL BONE. BY DR CUTHBERT.

The following case being one of an unusual character, has induced me to bring it before the Society:—

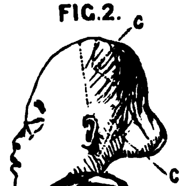
On the 18th October last, Mrs M'L. was delivered of a boy at the full time, her eleventh child. All her children were healthy and well developed, with the exception of two, viz., the ninth, which was still-born and premature; and the eleventh, the subject of the present remarks. My friend Dr Menzies attended my patient in her last confinement, and he was rather puzzled to make out the presenting part. His first examination led him to believe that it was a breech case, but the presenting part felt too soft for that. Then his next impression was that he had to do with an hydrocephalic head. The speedy termination of the labour, however, soon cleared up all doubt.

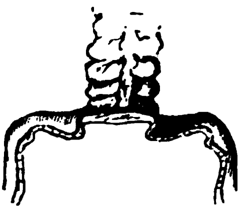
FIG. 1.



At the birth of the infant, the part marked A had a somewhat fleshy appearance, and membranous. The parts marked B were only slightly bulging; the extreme bulging, evidently caused by pressure of the brain against the scalp, did not take place till the second day. The membranous part at this date began to dry up, and it ultimately became quite black, except a small part which contained a quantity of fluid. On examining the back of the head, I could find no occipital bone; the head, as far forward as the dotted line C in the second illustration, was quite soft, apparently from brain matter, and unprotected by bone. I was afraid to push my inquiries very energetically into the base of the head, as, on making pressure, the child began crying from pain. But, so far as I could ascertain, the spine had an abrupt termination, as represented in the diagram.

FIG.2.





After the death of the infant, which took place on the eleventh day, I was allowed to make a more minute examination, which I did by pushing my finger well back into the neck, and tracing the edge of the bone till it joined the lowest part of the line C. The mother would not permit me to open the body.

The parietal bone did not extend further back than the line C. The edge of the bone passed slantingly down towards the angle of the jaw, where it joined the ridge of bone coming from the superior end of the spine. The frontal bones were normal, and the body was in every other respect healthy and well developed.

The expression of the face was very peculiar; the right eye had a slanting position, which gave it an Asiatic cast. This peculiarity was very much modified by the eighth day; the features then assumed more the ordinary appearance of infancy; the skin also became more pinky in colour.

The infant could not suck its mother's nipple. I examined the mouth with the view of ascertaining whether there was any defect, but there was great difficulty in opening the mouth; the jaw seemed as if partially anchylosed to the temporal bone. I could discover no defect. A nursing-bottle was provided, the nipple of which it sucked pretty well. It was also fed on sack whey.

I may mention, in connexion with this case, that, on the morning of the eleventh day, convulsions came on, which continued more or less all day, and the child died that night.

*Dr Keiller* said, it appeared to him the case was akin to spina bifida. He had a drawing not unlike that shown. His case is reported in the minutes of the Society. He thought it was a cystic tumour, and tried to operate. He opened the tumour, and found a quantity of fluid escaped. The cyst was double, as in these cases it usually is. Dr Cuthbert's case is one of incomplete development, occurring in the mesial line, as these defects generally do, both anteriorly and posteriorly. Cases of this kind generally die of convulsions. In the anencephalia the anterior part of occipital bone is generally present, and the case detailed to the Society is a modified form of the same condition.

IV. *Dr Young* communicated a paper on AN AID TO PARTURITION, AND TO THE TREATMENT OF DISPLACEMENTS OF THE UTERUS, BY A NEW MECHANICAL APPLIANCE, by Protheroe Smith, M.D., which will appear in a future number of the Journal.

*Dr Cochrane* thought that nature should be interfered with as little as possible. The accoucheur should interfere as little as possible in natural labour, and only in peculiar circumstances. Bandaging is an old practice, and had better be let alone.

*Dr Young* explained, it was only when nature failed that Dr Smith proposed to interfere.

*Dr Cairns* agreed with Dr Cochrane in his remarks, and observed that if Dr Smith's appliance would place the uterus in correct position he would give a case of retroversion of the uterus he had lately seen for trial.

*Dr Young* again explained, that it was only the gravid uterus which was referred to in Dr Smith's communication.

*Dr Bell* thought it would be well to have Dr Smith's instrument before the Society, and begged to request that gentleman to forward it to the Society before expressing an opinion upon its merits. On the general subject, he thought Dr Smith had overlooked an important matter, that, in savage life, the child's head was small, and also that the same variation in size was found in the agricultural districts of our own country. He also had overlooked placing the patient in a proper position, and had taken much for granted as to weakness of abdominal muscles. They are not so important as he seems to think, and, as a proof, he might state that in some instances the child is expelled by the uterine efforts alone after death; but he thought it would be unfair to express a decided opinion until the instrument was shown to the Society.

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### SESSION XXIX.—MEETING IV.

*6th January* 1870.—Dr BELL, *President*, in the Chair.

Dr Protheroe Smith, London, was elected a Corresponding Fellow of the Society.

I. *Dr James Young* showed an UMBILICAL CORD, measuring only ten inches in length. He had consulted various books on the subject, but could find no instance related where the cord was so short, and Sir James Simpson, who had seen it, said it was the shortest he had ever seen. The child was in such close contact with the mother that Dr Young had considerable difficulty in getting the ligature applied to the cord.

*Dr Cochrane* said he had met with a case of short cord, in which the labour was rapid, but the cord gave way near the navel. The child did well.

*Dr Bell* said that the usual opinion was, that shortness of the cord was a cause of tedious labour, but in Dr Young's case it did not seem to have been so.

*Dr David Gordon* said that in a case of so much shortening of the cord, the placenta would be drawn out of the uterus.

*Dr Cairns* said that, unless the placenta had been detached, he could not conceive how a labour could be so rapid as in this case.

### II. CASE OF OVARIAN DROPSY, WITH PREPARATION. BY DR R. PEEL RITCHIE.

*Dr Ritchie* stated he had alluded at a previous meeting of the Society to this case, as one he was watching by deputy, and in which certain variations had occurred in its progress.

The subject from whom the preparation was removed after death was Miss M. O. Although of respectable parentage, she became dissolute in her habits, and for many years led a profligate life; but having become legally insane, she was placed, in the year 1856, being then in her 38th year, in Bethnal House Asylum, near London, where she had been under his care whilst medical officer of that institution. She was then very noisy, violent, and obscene, intractable in disposition, and filthy in language and habits. Her examination was a matter of difficulty; and it was owing to her peculiar mental condition that she had been suffering from ovarian tumour for some time before the exact nature of her abdominal enlargement was ascertained.

In March 1866, there was such an increase in her size, that her condition then received more minute attention, and during the succeeding year she continued to enlarge; and not only on account of her enormous size, but of her general debility and habits, she was transferred in April 1867 to the Infirmary Department of the Asylum. Under the treatment of Dr Millar, Superintendent of Bethnal House, to whose kindness Dr Ritchie was indebted for the preparation and notes of the case, various palliatives were tried, but in the circumstances operative interference was not considered advisable. Her circumference at this time measured 54 inches, and the distention of the abdomen extended upwards to the xiphoid cartilage. She was also so feeble that her death was almost daily expected.

On the 19th of May an improvement in her condition is recorded, to the effect that her girth was reduced to 51 inches, and that her urgent symptoms had disappeared. On 4th of September it is stated she passed urine involuntarily as she sat about; and on the 9th of same month, that for the last two nights she had passed immense quantities of water. In consequence she was much reduced in size. One the 19th she was reduced to 41, and by 19th October she had diminished to 38 inches. After this she increased in size, and by 5th of November she had enlarged to 40 inches. She went on steadily increasing in size till 26th June 1868, when the girth was 49 inches. A change again took place, and on 16th July the dropsy had nearly disappeared. On 25th July the measurement was only 35 inches.

After this she enlarged till the time of her death, which occurred on 23d November 1869. It was preceded by gradual sinking and bilious vomiting. She died in her 51st year. The girth after death was 50½ inches, and the measurement from pubes to sternum was 23 inches. Dr Millar states "he carefully opened the peritoneal sac, and exposed the cyst, which was like an enormous bladder, and adherent all round, but very slightly to the peritoneum. The posterior part of the cyst was quite transparent; the fluid could be seen moving in it. The lower part of the cyst, particularly on the right side, had numerous large vessels running round it. The fluid amounted to five gallons three pints. It was limpid, quite clear, highly albuminous, and of sp. gr. 1015. I looked to see where and how the water could have disappeared, but could not satisfy myself; at the point apparently most likely I could not trace any opening."

In the remarks he made on the case, Dr Ritchie observed it was interesting in its psychological and its obstetrical aspects. He considered the immoral conduct of the woman before she became legally insane as of interest in connexion with the ovarian disease which subsequently became developed. Whilst leading her profligate life he thought she might be fairly considered to have been morally insane, and that her removal to an asylum would have been a justifiable and kind act, and he referred to the difficulty in deciding between moral insanity and wilful immorality. He thought her obscene language, gestures, etc., when in confinement in the Asylum, pointed to the ovarian irritation which existed. He alluded to the connexion between ovarian irritation and mental aberration, and to several authorities on the subject, but observed that the insanity was not invariably of the nymphomanic type, and detailed another case in illustration, which had been under his care. In that case the patient was melancholic and suicidal. In Miss M. O.'s case, he considered it probable that the ovarian irritation had existed for many years before she became the subject of general insanity, and he also believed that a hereditary tendency existed.

In its obstetrical aspects he thought the case was also interesting. The first distinct notice of her true condition was in March 1866, but long previous to that date her peculiar appearance had attracted notice, and it was not improbable she had been self-conscious of it, as she used frequently to say she was in the "family way." In April 1867, her condition was one of great urgency, which seems to have been connected with the tumour, and it gave a starting-point as to her size. An interesting point, too, was that her state at this time apparently indicated the commencement of the first reduction in size, for during the next month she became 3 inches less, and in September there was a more rapid decrease of 10 inches; with the reduction in the following month, there was consequently a decrease in the course of five months of 16 inches; and it was of importance to note that in September, when the rapid decrease took place, the only condition noticed was that she passed immense quantities of water. Had there been any injury to the cyst, some constitutional disturbance would likely have been produced. For eight months she went on increasing in size, till on 26th June she had gained the girth of 49 inches, or 11 more than in October. The second decrease occurred in July, and was very rapid, amounting to 14 inches in the course of a month; and from this time till her death, sixteen months after, she steadily gained in size, but never reached the girth when first measured in April 1867.

This diminution in circumference having occurred twice, might, he remarked, form the subject of considerable speculation, and that various theories to explain it might be advanced. The first point to settle was, whether there were more cysts than one. At first he was inclined to suppose there had been, and that one had ruptured on each of the two occasions alluded to; but after making a minute examination of the preparation, he could not satisfy himself as to there being any traces of a ruptured cyst, for he considered the cysts must have been of great size when they ruptured, the decrease being in the one instance 16, and in the other 14 inches. He felt satisfied the small thickened portion of the preparation could not represent a collapsed cyst of the required size. His speculations being limited, therefore, to the existing cyst, he arrived at the conclusion, that, although there were numerous traces of rudimentary cysts, one only had developed to great size. The next question which occurred was, How did the fluid escape from the cyst? He could detect no evidence of fistulous opening into the genital tract; and had there been one into the intestinal canal, it would not have been overlooked by Dr Millar. After several attempts, a piece of fine whalebone was passed along the right Fallopian tube. Supposing that rupture had occurred, there being *no* trace of fistulous opening, and only the right Fallopian tube being pervious, the matter was reduced to this, Could a tube of so small a diameter, that an ordinary probe could not be passed along it, and in which there was no evidence of previous dilatation, permit of such a flow of fluid along it as to cause the rapid diminution which took place, especially on the last of the two occasions? His opinion was, it could not. He could see no other way in which it could be removed but by the kidney. He referred to the observations of Sir J. Y. Simpson, Hewitt, etc., on the removal of ovarian fluid from the peritoneal cavity, but remarked that their observations were not applicable to the case before the Society, for in it there was no account of such a train of symptoms as accompany the sudden rupture of a large ovarian cyst, and the extravasation of its contents into the peritoneal cavity; and what was even more important, there was no trace in the walls of the cyst of rupture having occurred. Dr Thomas Keith had kindly examined the preparation, and was also of opinion that no rupture had occurred. He had seen several cases of a somewhat similar kind to that of Miss M. O., and had observed decrease of size, and its subsequent increase. The conclusion Dr Ritchie came to was, that the fluid had been in some way absorbed from the cyst directly, and without the intervention of rupture. In the cases in which Dr Keith had observed a similar decrease, the fluid, as in Miss M. O.'s case, was clear and thin. Dr Ritchie thought it was possible, and he might even say probable, that an endosmotic change had taken place by means of the large vessels, to which allusion was made among the post-mortem appearances. This was favoured by the extreme thinness of the posterior wall of the cyst, which was quite transparent, and by the limpid character and low specific gravity of the contained fluid. In such a view, it would be expected that when the distention of the cyst was greatest, the walls therefore thinnest, and the surrounding bloodvessels most stretched, that the transmission of the fluid would most likely commence; and referring to the case, it would be observed the decrease in size in the first instance began when the greatest distention had been attained, and the increase began when the reduction had reached 16 inches. In the second instance, the reduction commenced when the circumference had reached 49 inches; and when the lowest measurement of 35 inches was arrived at, the cyst reversed the action, and again filled. It would be instructive to have his view, on the one hand, confirmed, or, on the other, to have a more satisfactory explanation of the phenomena. He had only further to add, he understood Dr Keith to say he had rarely seen so small a pedicle, and that the case would have been an excellent one for operation.

Subjoined is a statement of the variations in size in Miss M. O.'s case:—

| | | | In circumference. | | |
|---|---|---|---|---|---|
| 1867. | April, | = | 54 | inches, maximum. | |
| „ | 19th May, | = | 51 | „ minus 3 | — 16. |
| „ | 19th Sept., | = | 41 | „ — 10 | |
| „ | 19th Oct., | = | 38 | „ — 3 | |
| „ | 5th Nov., | = | 40 | „ plus 2 | + 11. |
| 1868. | 23d Feb., | = | 45 | „ + 5 | |
| „ | 26th June, | = | 49 | „ + 4 | |
| „ | 25th July, | = | 35 | „ minus | — 14 minimum. |
| 1869. | March, | = | 45 | „ plus 10 | + 16½ |
| „ | April, | = | 47 | „ + 2 | |
| „ | May, | = | 48 | „ + 1 | |
| „<br>„ | June<br>July | = | 50 | „ + 2 | |
| „ | Aug., | = | 51 | „ + 1 | |
| „ | Sept., | = | 51½ | „ + ½ | |
| „ | 23d Nov., | = | 50½ | „ post-mortem circumference. | |

*Dr Duncan* said, that in his opinion one of the most interesting points in connexion with the history of Dr Ritchie's case was the rise and fall in the dimensions of the cyst, and the account given was of value as showing that diminution might take place in various degrees. Such a tumour may become dormant, for instance: the greater part of the fluid becomes absorbed, and what is left is transformed into a putty-like substance, resembling what is seen in old abscesses. Post-mortem disclosures prove that this is one way in which a cyst may diminish in bulk. It is alleged by some that a large ovarian tumour may be absorbed, cyst and all. Dr Duncan does not believe that such a thing ever took place. Thirdly, cysts may diminish in size by their walls giving way. He remembered a case in which the cyst burst twice and the belly collapsed : the woman suffered no pain. There is a statement made by authors, which has not, however, been often verified, namely, that a permanent opening may remain in the wall of a cyst. It is no doubt true that in post-mortem examinations and in operations ovarian fluid is frequently found in the abdominal cavity, which is the same as that contained in the cyst. This fluid may come out of the cyst, but Dr D. knows of no preparation which demonstrates this. Then, again, a cyst may empty itself by communicating with a mucous channel. Dr Duncan has known a large cyst burst high up into the bowel, and discharge gallons of fluid without producing any diarrhœa. The fluid passed into the colon and became absorbed. It is not necessary therefore to have physical evidence, in diarrhœa or otherwise, of the rupture of a cyst. The same remark is, so far, true in regard to diuresis and diaphoresis. He was of opinion that the first explanation might be made to account for the increase and decrease in the size of the tumour in the case related by Dr Ritchie. Dr Duncan has put on record a case where the fluid was discharged by the vagina, though how it got there no one could say. The patient suddenly found herself wet while in bed one day ; the tumour collapsed, and she became "neat," as she expressed it. The cyst re-filled, however, and she was operated on, but no adhesion of the cyst was found to any part of the genito-urinary organs ; perhaps in the re-growth of the tumour the adhesions were broken up and no trace of them left.

*Dr Bell* asked whether the previous history of the case might not lead to the supposition that the increase and decrease of size was dependent on ascites?

*Dr Ritchie* said that no fluid was found on examination in the peritoneal cavity.

*Dr Burn* was of opinion that, had ascites been present, it would have been diagnosed. He had had a case in which the cyst ruptured, and the woman suffered greatly from peritonitis. She is now, however, very well, and the tumour has not re-filled.

## III. FURTHER OBSERVATIONS ON A CASE OF VESICO-VAGINAL FISTULA. BY DR CHARLES BELL.

The patient having come to reside in Leith, I had an opportunity, in the month of July, of examining the condition of vagina and the urethra more carefully, and I found that the fistula was what is called the urethro-vaginal fistula. This explains how she was enabled to retain her urine for an hour or two when in the recumbent posture ; a circumstance which forms a remarkable contrast to what takes place either in the vesico-vaginal or in the vesico-uterine fistula, in which the urine continually dribbles away ; or to the case in which the opening into the bladder is above the insertion of the ureters, when "the flow of the urine takes place while the patient lies, and ceases when she stands."[1]

The occlusion of the urethra was occasioned by the adhesion of the lining membrane forming as it were a kind of diaphragm, which could be pressed towards the bladder by a probe or catheter, when it was easily felt by the finger in the fistulous opening. This induced me to puncture it by means of a sharp probe. I then passed a catheter into the bladder, and I intended to retain it there, but it produced so much pain and irritation that it was necessary to withdraw it. In order, therefore, to preserve the urethra pervious, I passed a cord along it, and brought it out at the fistula, and tied it. Finding that it produced no irritation nor annoyance, I allowed it to remain until the month of November, when she came to the hospital to be operated on.

On the 15th November, Dr Keiller operated for the fistula, with his usual skill and success. The case went on satisfactorily. The stitches were removed on the 22d November, and she was dismissed cured on the 29th. She returned to the hospital on the 6th December, and stated that she was quite comfortable, and that she had been able to go to church.

*Dr Burn* asked how the fistula had been produced, and was of opinion that many cases of the kind were caused by neglecting to use the catheter.

*Dr Bell* said that in his case the patient had had a tedious labour—forceps had been used, the child's head was broken up, and then things were left to nature. The fistula must have been caused either by pressure on the urethra or from an injury inflicted by the instruments. It was often a matter of some difficulty to pass a catheter, owing to the change in direction of the urethral canal. It was of great importance in all manipulation of the female passages to avoid introducing infectious matter by the hand, and he had recently been in the habit of using a liniment which prevented the possibility of this, and at the same time destroyed smell. Its composition is permanganate of potass and pure glycerine, 4 grains to the ounce.

*Dr Pattison* mentioned that he had seen a case where a fistula was caused by not introducing the catheter.

*Dr Bruce* said that carbolic soap was a useful disinfectant and deodorant.

*Dr Taylor* wished to know how the occluded urethra in Dr Bell's patient had been reopened.

*Dr Bell* said he had punctured the occluding membrane, and formed a new urethra.

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[1] Thomas on the Diseases of Women, p. 156.

# Part Fourth.

## PERISCOPE.

### SURGERY.

A CONGENITAL OMENTAL HERNIA MISTAKEN FOR A HYDROCELE, AND TREATED BY PUNCTURE.—A young man, aged 18, entered M. Demarquay's Clinique, having been sent by M. Ricord to be operated on for a congenital hydrocele. He had been seen by other surgeons who had taken the same view of the case.

His parents, without being very precise as to the date, believed that the tumour had existed from childhood, and the surgeons who had seen him then had had no doubt as to its being a hydrocele.

The patient had never suffered from the tumour, except a little uneasiness after fatigue and vague pains occasionally in the lower part of the abdomen. The tumour was pyriform, about the size of a turkey's egg; it was not prolonged up the cord, which seemed no larger than the opposite one. It was soft, fluctuated, was dull on percussion, and perfectly transparent. The position of the testicles was easily made out by their opacity. On pressure on the scrotum, the tumour was easily made to disappear without any gurgling. If the fingers were kept applied to the inguinal ring, and the patient asked to cough, the swelling became reproduced, and fluid was felt to pass the fingers.

The diagnosis of congenital hydrocele was made, the operation fixed for the 24th of February.

*24th Feb.*—To-day the patient walked about a little before the visit, so that all the fluid might accumulate in the tunica vaginalis. M. Demarquay intended, after puncturing the tumour, to inject it with iodine. The fingers of an assistant were applied firmly to the inguinal ring, to prevent any of the iodine from penetrating the peritoneal cavity. However, on inserting the canula, and removing the trocar, not a single drop of fluid escaped. On removing the canula, a small piece of omentum was seen to project, and then return. The patient was put to bed, and the tumour fomented. For the next few days, the tumour was again seen to be quite transparent, and on 2d March, M. Demarquay again punctured the tumour, and this time obtained about ten drachms of a clear, slightly greenish fluid. As some transparency still remained after this evacuation, another puncture was made, which obtained only a little blood. The tumour was now softer, and diminished in size, but during the rest of the month the patient suffered from a smart peritonitis, after which he returned home not much better than he was before.—*Gazette des Hôpitaux*, No. 89, 1869.

RANULA CONSISTING OF A HYDATID CYST.—A man, aged 62, has for some time had a troublesome tumour, involving the left side of the floor of the mouth. It forced the tongue upwards and backwards, and interfered with swallowing and pronunciation. Dr Homolle had punctured it, and passed a small seton, but the thread escaped; on the fifth day the wound closed, and the tumour began again to enlarge. It had attained the size of a small hen's egg, and filled up the buccal cavity.

M. Gosselin examined it, recognised fluctuation, and diagnosed ranula, with, however, two unusual concomitants,—a thickening of the buccal wall of the cyst, and an enlargement of some of the submaxillary glands. Did these two conditions depend on the previous treatment, or were they the result of some epithelial growth on the inner wall of the cyst? These questions were answered at the operation, which M. Gosselin executed in the manner he prefers, *i.e.*, a long incision, excision of the cyst, followed by repeated cauterizations. On making the free incision, a quantity of pus escaped, and the large white enveloping capsule of the mother hydatid came out, and in it, on microscopic examination, were found numerous entire echinococci, and a large number of cephalic hooklets. There was no possible sign by which this very remarkable case could have been recognised before operation. No unpleasant symptom has followed the operation, and there seems every reason to believe that the patient will make a speedy and complete recovery.—*Gazette des Hôpitaux*, No. 55, 1869.

CAUDAL APPENDAGE IN A NEWLY-BORN INFANT.—A male infant, five weeks old, was lately treated by M. Gosselin, at the Surgical Clinique at the Charité. He had in the middle of the gluteal fissure an appendix rather more than two inches in length, and thicker than a goose-quill. This appendix, rosy in colour, and quite destitute of hair, resembled the rather elongated penis of an infant, but it had no opening at its end. When at rest, it lay coiled up in the hollow between the buttocks, and concealed the orifice of the anus.

In considering the question as to the propriety of operation, M. Gosselin carefully examined its connexions in case it should be found to be a prolongation of the coccyx, and a sacrococcygeal canal; in which case any operation would run the risk of inducing spinal meningitis; however, it was felt to be completely soft, and presented no trace of any osseous deposit in its whole extent. Its attachment, too, was perfectly free, showing no adhesion to any part of the bony skeleton. It was removed by two semi-elliptical incisions, and was found to consist simply of fibrous tissue containing fat, and covered in by a thin layer of skin.—*Gazette des Hôpitaux*, No. 55, 1869.

RESECTION OF THE KNEE BY A NEW METHOD, BY M. SEDILLOT OF STRASBOURG.—We can only here insert the briefest possible abstract of a very long and rather tedious account of this new operation and its result.—(*Translator.*)

After an account of the dangers of this operation in adults, and its comparative safety in children, M. Sédillot states that in children it is very rarely necessary, as nature possesses such resources, that cures can be almost certainly obtained by proper treatment, without either excision or amputation, so that in his own practice he has very rarely met with cases suitable for this operation. At last a good case of white-swelling presented himself, aged 21, of good constitution, with disease not too far advanced, and yet not likely to recover without operation.

A case under the care of M. Boeckel had suffered from such severe muscular contractions, that it was found necessary, notwithstanding the use of chloroform, to cut off a second slice of bone, before the cut surfaces could be adjusted. M. Sédillot had been considering the question, whether, under similar circumstances, hooks, or pins of iron or ivory, might be useful in maintaining the position of the bones. However, he at last thought that he had at his disposal a safer and more certain plan. This was to dovetail the cut surfaces to each other, by cutting the end of the femur into a wedge, which should be received into a corresponding hollow to be cut in the tibia.

This was done with rapidity, and without difficulty. However, an unforeseen complication gave much trouble. Alarming hæmorrhage from all the four surfaces of bone caused a considerable loss of blood, notwithstanding the use of compresses, sponges, agaric, eau di Pagliari, and the actual cautery. At last it was checked, after at least an hour's labour, by the application of numerous pads of lint soaked in perchloride of iron, which certainly did not improve the condition of the cut surfaces of the bones. Eventually, however, they were brought together and fitted admirably.

Next day, after considerable chloroform vomiting, and little sleep, the bandages were changed, and the wound dressed with a *digestive*.

For three days, excessive discharge of black coagula and detritus; and on the fourth day after the operation he had a violent rigor, the herald of a rapid and severe attack of pyæmia, which carried him off on the seventeenth day after the operation, and two days after amputation through the thigh.—*Gazette des Hôpitaux*, Nos. 66, 68, 1869.

PERINEAL LITHOTRITY.—M. Dolbeau made the following remarks on this subject:—In the month of January 1863, I performed for the first time in Paris an operation to which I believe the name of perineal lithotrity should be given. My patient was rapidly cured, and last August he made me aware that he had not forgotten me. Since that date, I have practised the operation twenty-one times. All my patients recovered, except the one last operated on.

Up to this date, this operation had been performed only by myself; and all my efforts to persuade my colleagues to imitate me itherto been unsuccessful.

To try to convince surgeons, I intend to publish a full account of my operations. To-day I only wish (preparation in hand) to show that this operation is one conducted on exact principles, and that certain propositions which I advanced in my treatise on Stone, are to-day confirmed by the autopsy.

My patient, aged 67, suffering from a large and very hard stone, went on admirably, till, on the eighth day after the operation, he showed distinct signs of acute mania. On the night of the twelfth day, he was found hanged by the cord of his bed, and on the fifteenth day he died, after having refused all food for a week.

This certainly is a case of death after operation, but which cannot be ascribed to the operation only.

The following are, in a few words, the different stages of perineal lithotrity:—

1. An incision through skin and cellular tissue never exceeding ten lines in length, just in front of the anus, and exactly in the middle line.
2. Puncture by the catheter in the posterior angle of the wound.
3. Gradual dilatation of the wound made by the catheter.
4. Dilatation of the neck of the bladder to the full extent of the instrument,—that is, to about ten lines in diameter.
5. Breaking of the stone, and removal of numerous fragments.

The operation had been performed in the usual manner, and everything promised success, till, from various circumstances, the patient died on the fifteenth day after the operation. The autopsy was made with great care, and now it is my duty to show the importance of the facts revealed by dissection.

The preparation shows the following points:—

1. The bladder is natural in size; the walls are thick, and very muscular; the mucous membrane is white, soft, presents no ulceration, no laceration, no abnormality in colour, either the result of inflammation, or even indicative of the presence of blood in the subjacent tissues.
2. The neck of the bladder—and to this I would wish to draw special attention—is normal, though the patient was 67 years old, the urethro-vesical orifice preserves its circular form; the mucous membrane lining its inner surface is entire, and presents no sign of laceration or bruising. It is necessary to lay open the orifice to see the tear of the membranous portion through which the dilator was passed. I repeat, the neck of the bladder is intact, so much so, that at the autopsy the bladder was found full of water.

The state of the urethra and its appendages, shows (1.) The urethra is intact from the meatus to within a line of the bulb; from which spot (2.) a linear incision in the middle line extends to the neck of the bladder. (3.) The bulb of the urethra is healthy and normal, the incision not having interfered with that organ. (4.) The muscles of the perinæum are all healthy, and the vessels show no trace of either wound or inflammation.

In a word, the preparation proves that perineal lithotrity is a legitimate operation, performed by rule, and justifies the principles on which the operation is based. 1. The possibility of making a regular passage from the anus to the bladder without interfering with the bulb, rectum, neck of bladder, or vessels. 2. The possibility of introducing instruments to break the stone without destroying the physiological action of the neck of the bladder.—*Gazette des Hôpitaux*, No. 2, 1870.

REMOVAL OF UTERUS AND ITS APPENDAGES.—The following short notice of a remarkable operation, performed by M. Pean, the subject of which was shown to the Academy of Medicine in Paris, at its meeting on December 8, 1869, is from the pen of Dr Revillout:—

The tumour is multiple, containing, 1. A very large cyst of the left ovary. 2. The uterus itself hypertrophied, containing a cyst, and extending up as far as the umbilicus. 3. A fibrous tumour of the right ovary, and a cyst of the right Fallopian tube. . . . Before the operation, M. Pean had diagnosed the existence of a movable fibrous tumour, independent of the ovarian cyst. He had also made out by vaginal examination that the uterus was hypertrophied, but he did not know to what amount, and could not make this out on account of the adhesions which bound the tumours to each other.

When the cyst of the left ovary became visible through the incision in the abdominal wall, M. Pean began to remove it in his usual manner, stopping the hæmorrhage by long cauteries, heated to whiteness. After having got out a large part of the cyst, he arrived at the uterus, which he found to be greatly enlarged, soft, fluctuating, and everywhere adherent. These adhesions were very vascular, and could not be broken down without excessive hæmorrhage, which even the cauteries failed to arrest. In such circumstances, no resource remained but to remove the diseased uterus along with the other tumours. This M. Pean did not wish to do, through the neck of the uterus, as it, excessively hypertrophied, was as large as a fist. He preferred to carry the section through the vagina itself, and he succeeded in passing across the vagina from before backwards, and through the abdominal wound, a double thread, by which he tied the tumour in two halves; the one on the left side secured the great ovarian cyst, the other one included all the vagina corresponding to the uterus, and isolated, besides that organ, the right ovary and Fallopian tube. After having tied the two ligatures, M. Pean cut off all the tumours close above them, and then by strong pulling brought the pedicle (such as it was) to the abdominal wound. This he did not close at a point opposite to the pedicle. Three india-rubber tubes were passed down to the bottom of the cyst to allow escape of discharge. A good recovery took place.—*Gazette des Hôpitaux*, No. 143, 1869.

# Part Fifth.

## MEDICAL NEWS.

**Relapsing Fever** has at last broken out in Edinburgh in two different and distant parts of the city, without, we believe, any contagious influence being traceable in either case. The history of this outbreak, carefully recorded, as we hope it shall be, must form one of the most interesting episodes in the modern history of fever, as well as a not unimportant contribution to the vexed question of change of type.

**Death from Chloroform.**—One of these lamentable accidents recently occurred in the Dispensary at Alloa. A full account of it is about to be communicated to the Obstetrical Society of Edinburgh by Dr Brotherston, in whose practice it occurred, and will thereafter appear in our pages.

**The Royal College of Physicians, Edinburgh, and the Lancet.**—In consequence of recent animadversions by the *Lancet* upon the conduct of the College in regard to the manner in which it bestowed its license to practise at the time when that license was instituted, the Council have thought it right to lay before the licentiates of the College a statement of the facts of the case. This they have done by re-publishing an "Explanatory Minute," issued by their predecessors in office, setting forth most clearly the motives which influenced the College in electing its licentiates at that time. This statement has remained unanswered, and is, in the opinion of the Council, unanswerable. It is too long to re-publish *in extenso*, but its leading features are as follow:—

I. The College disclaim any assumption on their part that their licentiates have any right to the title of Doctor of Medicine. Such an idea could only have arisen from the fact that the College of Physicians of London at one time sanctioned the claim of its non-graduated licentiates to the title of Doctor. No similar claim has ever been in any way put forward by the College of Edinburgh.

II. The College meet the charge of having compromised the dignity of a College of Physicians by granting its license to general practitioners, by showing that such peculiar views in regard to their dignity have never been held by them, but that for thirty years and more they have been endeavouring by negotiations with the Royal College of Surgeons of Edinburgh, whose diploma is that chiefly held by the general practitioners in Scotland, to extend the benefits of their license to general practitioners. And they also show that the diverse and more restricted views ef the London College of Physicians have had the effect of forcing the large body of general practitioners in England into connexion with a mere trading association, the Apothecaries' Company, entitled only to charge for drugs, and having no right to any remuneration for strictly medical advice and attendance,—a circumstance which has had a most injurious effect upon the social position of the general practitioner in England. In Scotland such a deplorable result was only averted by the broad views adopted by the College of Surgeons, whose licentiates were all carefully examined in medicine, and whose license was therefore actually that of a general practitioner. The Medical Act of 1858, however, by requiring medical and surgical qualifications to be separately registered, virtually declared that the license of a College of Surgeons should no longer be sufficient for general practice, at least as regards those holding appointments implying medical duties. The Edinburgh College of Physicians, therefore, in these novel circumstances, determined to act in accordance with their own established precedents and their carefully considered views of sound policy, and to grant a separate license in medicine, so as to place a medical qualification within the reach of every well-educated medical student in every school in the kingdom. By thus identifying themselves with the interests of the profession at large, the Edinburgh College has followed a course clearly marked out by legislation, and one which must be followed, at no long interval, by the sister Colleges.

III. In regard to the proceedings of the College in the so-called "year of grace," the College have shown that this was no invention of theirs to fill their coffers, as has been alleged, but was simply a necessity arising out of past exclusiveness, and was first of all announced and adopted by the London College. The London College, however, in accordance with its peculiar views, restricted its grace to University graduates. The Scotch College, with its broader views, regarded such a restriction as a simple act of injustice to the general practitioner who found himself necessitated by the Medical Act to procure a new qualification at a time of life when he could hardly be with propriety asked to become again a student, or to pass any further examination. To meet this difficulty, the Edinburgh College agreed, under certain circumstances, to confer their license during a limited period without that examination, afterwards to be indispensable, indisputable evidence of character and qualifications being, however, in every case required. This resolution was subsequently rescinded, in so far as that candidates were subsequently required to pass an examination in medicine, or to produce proof that they had done so. This document concludes with the following quotation from the *Lancet* of 13th August 1859:—

"The Council of the Royal College of Physicians of Edinburgh are just now fighting the battle of common sense in the profession. They have taken up the sword for the *plebs;* and if they wield it manfully, and with frank and valorous mien, there can be no ques[illegible]ion of the result. They have offered to the general practitioner of nited Kingdom a means for placing himself on a professional o far as title and degree can go, with the magnates of the profession. It is very right that such a door should be opened; but it is also right that only those who are worthy, and can approve themselves fit companions, be admitted. Of the cold exclusiveness which would affirm that the designation of doctor or physician is inapplicable to the general practitioner, we can but express our profound disapproval. It is the function and the duty which hallow that title; and to assume that the licentiate of a college, the member of a corporation, or graduate of a university, robs himself of the right to the name of doctor or physician by practising the general duties of his profession, instead of confining himself to consulting practice, is an absurdity which is not likely to be ratified by the general voice of the profession, and one to which the public will never practically give confirmation. The fact is, that the distinctions between graduates, members, and licentiates, of the various corporations, are very fairly represented by the respective letters which these gentlemen are authorized to place after their names; and the general term of doctor is one which public usage will long continue to bestow upon every member of the medical profession. The Council of the Edinburgh College hint at this very intelligibly in their explanatory minute. The Council will be borne out by the great body of the profession in their recent measure, so far as it tends to bring in unison with them a larger number of their *confrères*. The tendency of the day is towards a general union, and the creation of one faculty. Those who aim at eternally perpetuating differences and distinctions of an arbitrary nature, do not comprehend their own interests or those of the profession."

MORTALITY AS DEPENDENT UPON DEFECTIVE HOUSE CONSTRUCTION.—A lecture on this important subject was recently delivered by Professor Gairdner before the Philosophical Society of Glasgow. In this he pointed out the causes and evils of overcrowding, and why this should be so much more injurious in towns than in country districts, inasmuch as in the former it was not merely the apartment but the area that was overcrowded. Dr Gairdner insisted that eight points were essential to the proper construction of a house: 1. Adequate cubic space in the sleeping apartments; the only way of providing this being, in his opinion, to throw the responsibility for overcrowding upon the owners, and not, as now, upon the occupiers. 2. Proper means of separation and privacy for the sexes within the houses; and in regard to this, while prescribing one-roomed tenements as wholly inadmissible in a well-regulated town, he yet advocated their permission in such exceptional cases as of a newly-married couple, an old couple whose children had left them, or in the case of widows. 3. Proper means of access; in speaking of which he adverted to the horribly filthy and injurious plan of closing-in common stairs at the top, and ventilating all the water-closets in the tenement into them. 4. Proper lighting as well as ventilation of both rooms and passages. 5. Adequate privy accommodation. 6. Adequate water supply. 7. Baths and wash-houses. 8, and last. Proper airing and recreation ground. This lecture, which will be found *in extenso* in the *North British Daily Mail* for the 10th February, points out evils which exist in Edinburgh no less than in Glasgow, as has been well shown in a recent number of the *Builder;* and, in so far as it suggests remedies for such defective sanitary conditions, is well worthy of the attention of those reformers who seek to improve the wellbeing of the people, and desire to reduce the enormous mortality of our city.

DEATH OF DR ANDREW ANDERSON OF GLASGOW.—This gentleman, who occupied the prominent position of President of the Faculty of Physicians and Surgeons, died on the 28th January, in the fifty-third year of his age. Dr Anderson was Professor of the Practice of Physic in the Andersonian University for nearly twenty years. He is well known to the profession by his work on "The Study of Fever," as well as by many other practical contributions to the science of medicine, many of which appeared in our Journal. Dr Anderson was rapidly obtaining the position of one of the leading consulting physicians in the west of Scotland, and his death is deeply deplored by the profession and the public, as well as by his own more immediate personal friends.

## COMMISSIONS IN THE BRITISH MEDICAL SERVICE.

LIST of Thirty-eight Candidates for H. M.'s Indian Medical Service, who were successful at the Competitive Examinations at Chelsea in August 1869, and at Netley in February 1870, after having passed through a Course at the Army Medical School. The Candidates are arranged in order of merit. *Maximum*, 6900.

| *Names.* | *Studied at* | *Marks.* |
|---|---|---|
| Duke, O. T. | London, | 5518 |
| Nicholson, F. C. | Edinburgh, | 5440 |
| Gunn, J. S. | Aberdeen, | 5005 |
| Hendley, T. H. | London, | 4915 |
| Gregg, W. H. | Dublin, | 4600 |
| Seaman, A. B. | London, | 4508 |
| Ghose, F. C. | Calcutta & Edin., | 4451 |
| Smyth, F. A. | Dublin, | 4370 |
| Sinclair, D. | Aberdeen, | 4250 |
| Salaman, S. M. | Dublin, | 4230 |
| Boyd, H. | Boston & London, | 4025 |
| Lloyd, J. | Cork, | 3975 |
| Barker, F. C. | Dublin, | 3913 |
| Courtney, W. M. | Dublin, | 3860 |
| Ruttledge, E. B. | London, | 3736 |
| Robinson, T. | Dublin, | 3715 |
| Caldecott, R. | London, | 3694 |
| Martin, D. N. | Cork, | 3648 |
| Strahan, A. B. | Aberdeen, | 3612 |
| Roe, W. A. C. | Dublin, | 3607 |
| Kelly, A. H. | Dublin, | 3575 |
| Meadows, C. J. W. | London, | 3555 |
| Waters, G. | Glasgow, | 3478 |
| Keefer, W. N. | Montreal, | 3460 |
| Deane, A. | Dublin, | 3392 |
| Murphy, P. | Cork and Dublin, | 3365 |
| Murray, W. F. | Dublin, | 3196 |
| Carswell, J. S. | Glasgow, | 3166 |
| M'Conaghy, W. | Dublin, | 3046 |
| Hughes, A. H. | Toronto & Lond., | 3027 |
| Wall, R. M. | Dublin, | 2940 |
| North, J. | London, | 2770 |
| Fawcett, E. | Dublin, | 2703 |
| Paterson, F. R. | Dublin, | 2697 |
| Hastings, W. | Dublin, | 2683 |
| Spencer, T. C. H. | London, | 2679 |
| Hall, H. G. | Dublin, | 2435 |
| Jones, H. J. | Glasgow, | 2431 |

# Part First.

## ORIGINAL COMMUNICATIONS.

### ARTICLE I.—*On the Construction of Hospitals.* By Dr WATSON WEMYSS, F.R.C.S.E., etc.

THE following lecture on the Construction of Hospitals for the Sick and Hurt, delivered at the Conversazione of the Royal College of Surgeons, on the evening of the 17th of February, by Dr Watson Wemyss, F.R.C.S.E., formerly a practitioner in Edinburgh and one of the Surgeons of the Royal Infirmary, but now retired from active practice, will be read with interest, as it takes a comprehensive view of the whole subject, and particularly as it relates specially to the medical profession.

MR PRESIDENT AND GENTLEMEN,

I have undertaken to lay before you this evening some remarks on the construction of hospitals for the sick and hurt,—comprehending in this short phrase (of sick and hurt) the cases of disease and injury, which, by conventional usage, are distributed into the medical and surgical wards of a general hospital.

I. At a time when there is an increasing demand for the erection of hospitals and other large buildings for the care of the sick, the hurt, the insane, and the destitute, as also for the education of youth, and the amusement of the people, the proper construction of such buildings is both important and interesting; and it becomes a matter of special concern that these should be formed and arranged, not only in the best manner for their several destined purposes, but, from the increasing wants of the people, and the many new requirements which science and art have shown to be necessary for the health, comfort, and benefit of their inmates, as well as from late disclosures, pointing out the defects and danger of such buildings, their construction and internal arrangements demand more particular attention than has hitherto been bestowed upon them.

In all civilized countries there are hospitals for the sick and hurt, and their construction has engaged the particular attention of those who, from their position in society, were required to conduct their management, and also of the medical men who had charge of the patients admitted into them. But it has been from the experience and observation of the medical men that a knowledge of the influence, arising from their construction, upon the health of their inhabitants has been acquired. This has led to many improvements in their form, construction, and internal arrangements.

The remarks I have to lay before you relate chiefly to general hospitals for medical and surgical cases. They are derived from theory and experience combined, not of myself only, but from a review of what has been done by others also, over a very wide field of observation. [Allusion was here made to the enlarged plans of several of the most celebrated hospitals, prepared and hung up for the better illustration of the subject.]

I may also add, that my remarks will be entirely of a general nature, as they are intended to show the general principles upon which hospitals should be constructed, but without reference to any particular place.

I purpose to describe to you shortly the different forms of hospitals which have been erected in this and other countries, and to explain the advantages and disadvantages which seem to belong to their construction and internal arrangements. But before doing so, I must take leave to make a few general remarks on the principal objects and requirements now established as necessary in the construction of an hospital.

## II.—*Of Plans for Hospitals.*

Experience, as well as medical science, teaches us that the health and lives of those confined in hospitals depend much, not only on the situation in which they are placed, but also on the size and construction of these buildings. Hence the interest which medical men (including physicians, surgeons, and general practitioners) are called upon to take in the selection of a site, and in the adjustment of the plans prepared for hospitals. And hence, also, the necessity for the plans being arranged by the combined experience and skill of medical men and architects. The former specify what is required, as regards the position, size, form, and internal arrangements which are necessary for the efficiency of the hospital; the latter have to devise the best and most economical manner for having these carried into effect, and to get them executed. In doing this, the architect has abundant opportunity to exercise his ingenuity, taste, and skill, in constructing and ornamenting buildings destined to fulfil the most noble purposes. In these special departments of the architect, I may take upon me to say, neither physicians nor surgeons will interfere.

The instructions to the architect, therefore, should be given at the commencement by the medical men. If otherwise, how is the architect to proceed? He may prepare plans according to his own knowledge of the subject; but of these the doctors may disapprove, and hence an unnecessary, unpleasant, and unseemly contest is created between the doctors and the architect, perhaps ending at last, as in too many cases it has done, in an imperfect building being [illegible] giving little or no satisfaction.

In consequence of the advice of medical men having been either ignored or only partially acted upon, many of our large hospitals and asylums have not been so constructed as to be most suitable for their purposes and the health of their occupants, thus occasioning not only disappointment, but great additional cost for subsequent alterations, which too frequently have only lessened the evils experienced.

But surely the medical profession should know the best form of an hospital, and the best size and arrangement of its wards, for the health of the patients, and such other arrangements as are most suitable and convenient for the physicians, surgeons, their assistants, and other officials of the institution. And surely those who have laboured, and who are still to carry on their important work in the hospital, are, from experience, the best judges of what is needful, most beneficial, and most convenient for all concerned.

Hence it is, that neither by the architect alone, nor by a medical man, can a proper plan for an hospital be formed; but only by the combined experience and skill of both. And much labour and trouble would be saved, by the needful instructions to the architect being given at first by the doctors.

### III.—*Of the Principles upon which Hospitals should be constructed.*

From what has been said, it therefore becomes a very important matter for the public, that those well-ascertained, definite, and established principles which have been acquired by experience, and have obtained the general approval of the medical profession, and upon which hospitals should be constructed, should be known and kept in view by those who are employed in preparing designs and plans for them. And it is no less important, also, for the medical men, who have the weighty responsibility of the proper care and treatment of the patients admitted into them, to see that these principles are properly carried into effect.

(1.) *Of the Site for an Hospital.*—The first of these principles to which I shall allude is the selection of a site for an hospital. This should be an airy, dry, and somewhat elevated situation, well exposed to receive the rays of the sun, and sheltered from the most hurtful winds which prevail. It should be well supplied with good water, and possess proper and efficient drainage. And it should not only be of sufficient extent for the buildings required, but also for airing-grounds for convalescent patients, as this would also secure more open space for the buildings.

(2.) *Separate Buildings for the Medical and Surgical Departments* of a general hospital, distinct from each other, are now considered, if not absolutely necessary, at least to be a great improvement in its arrangements. The cases of the various diseases and injury which present themselves for admission into an hospital, being easily separated and assigned to the physician and surgeon respectively, with sufficient accuracy for treatment and clinical instruction, it is importance that the medical and surgical wards should be in separate buildings. The great advantages to be derived from this arrangement are—*first*, that the number of patients under one roof will be lessened; and, *second*, that the extension of contagious diseases to patients under surgical treatment for injuries and other local complaints may be prevented.

(3.) *Separate Divisions for Male and Female Patients.*—It is scarcely necessary to say that each of the two departments of which I have spoken have again to be divided for the separation of the sexes. Hence a general hospital, consisting of medical and surgical departments, requires two sides or divisions, one for the male, the other for the female wards, thus forming four separate divisions, to which I shall afterwards have occasion to refer more particularly.

(4.) *Hospitals should consist of Two Floors only.*—Hospitals should not consist of more than two floors, or stories, above a basement floor. Long stairs leading up to wards higher than this are extremely inconvenient and disadvantageous in many respects, by being too distant from the apartments of the resident officials and other parts of the hospital. Besides, as the foul heated air from the lower parts of a building ascends to the higher parts, the concentrated foul air of the lower wards ascends to those above, so that by limiting the building to two floors the noxious circulation and effects of this foul air may be much lessened. Other reasons for this limitation, connected with the pavilion system, will be afterwards stated.

(5.) *Size of the Wards of an Hospital.*—As you all know, life and health cannot be maintained without a constant supply of fresh air, which, in confined situations, requires to be very frequently renewed, more especially in the case of sick persons confined within the wards of an hospital.

This circumstance, therefore, constitutes one of the chief objects to be secured in its wards, and is to be accomplished—

1*st*, By the air space provided for each patient being at least from 1000 to 1200 cubic feet;

2*d*, By the properly proportioned breadth, length, and height of the wards; and,

3*d*, By efficient means being provided for ventilation.

The form of wards may be either square or oblong; if square, their size would be very limited, because, there being no occasion for a ward being more than 26 or 28 feet in breadth, this would form the size of the ward made square.

The size and proportions of a ward are not now arbitrary, certain data founded on experience having been fixed, by general approval, for their dimensions. These data consist of the extent of air space and floor space for each bed, requiring certain proportions in the breadth, length, and height of the ward. The result is, that, as a medium, the air space for each patient should be at least 1000 or [illegible]0 cubic feet, and the floor space for each bed about 90 square feet. Hence, a ward of an oblong form, having a row of beds on each side of it, and of a medium size, should be 26 feet in breadth, 15 feet in height from the floor to the ceiling, and 7 feet in length for every two beds opposite to each other. These dimensions secure for each patient 1365 cubic feet of air space, a sufficient separation of the beds, and ample room for the attendants to go about; and the total length of a ward will depend on the number of beds it is intended to contain,—7 feet of length being added to it for every two opposite beds. These dimensions cannot well be departed from, or lessened, to any considerable extent, without injurious consequences as to efficiency.[1]

A knowledge of the proper size for wards is important to medical men, in order that they may see that they are not too much crowded with beds. The extent of space considered necessary for each individual is a matter of importance also to the community, as showing what the size of their apartments ought to be.

(6.) *Ventilation and Heating of the Wards.*—The free circulation of fresh air or ventilation of the wards of an hospital being another matter of the utmost importance, requires much attention from those concerned in their construction.

There is too much reason for the opinion that, when sick or injured persons are confined together within the wards of an hospital, the contained air not only becomes vitiated or foul from respiration, but there is also a peculiar *malaria* engendered, which is very detrimental to the health of all within the house, unless the most ample and efficient means are provided to counteract these effects, by the constant renewal of the air contained within the wards from without. The very serious consequences produced by this *malaria* have formed the chief objections which have been urged against large hospitals.

To effect the complete and proper ventilation of a ward, windows are required on both sides of it, opposite to each other, and not on one side only. And the windows should be so constructed that, when required, fresh air may be admitted without injurious draughts upon the patients. Windows for hospitals have been constructed in many different ways, with ventilators and other appendages; but none seem better adapted for the ingress of fresh air and the exit of foul air, as also for easy regulation and management, than our common sash windows, made to open both above and below, and provided with some additions (as of perforated glass or zinc), to prevent direct draughts upon the patients.

In this climate the windows should not be too numerous; one window between every two beds may be considered quite sufficient.

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[1] The floor space and air space for each bed is easily calculated, by multiplying the length and breadth for two beds together for the floor space, and then multiplying the product of these by the height of the ceiling for the air space; by dividing the result by two will be the dimensions for each bed.

The heating and ventilation of hospitals may be made serviceable to each other, and should, therefore, be considered together.

By attempting to effect these objects by complicated constructions and machinery, disappointment has been the general result; so that the simplest means—*i.e.*, the proper construction and regulation of the windows and open fireplaces—have been reverted to, being found to answer best. By these means, what has been termed "natural ventilation" is secured, generally found sufficient, easily regulated, and is a saving of cost.

But when wards are very large, and in cold weather, other auxiliary means, such as stoves and hot-water pipes, may be necessary for heat and ventilation. And one of the best and safest modes of using these consists in placing them in the lobbies, passages, and corridors; and along with this, by placing *louvres* above the doors, and ventilators at the roof, the air of these parts may be kept pure and sweet, and being heated, may be admitted into the wards at night in cold weather with great advantage, when open windows would be unsafe.

Cases showing the injurious consequences which have been caused by the absence of open fireplaces and opposite windows in wards, and the salutary effects from these being supplied, might here be adduced, but I forbear to occupy your time with such details.

The baneful effects of the peculiar foul air of an hospital on the health of its inmates, have been termed "Hospitalism." Of these bad effects in many large hospitals, arising either from defects in their construction or management, there can be no doubt. But upon this it is not my intention to dilate, leaving this part of the subject to the much more able hands of our distinguished friend, Sir James Simpson, who has done so much to elucidate it, and who has collected and contributed so much valuable information regarding it, as to lay the profession and the public under very deep obligation; because what has been brought to light forms and places before us a beacon, warning us against a prevailing cause, which prevents the success of the best applied medical and surgical treatment, but which may be either lessened or altogether removed.

Do not suppose, however, that I have any desire or inclination to say anything in the least approaching to a general condemnation of hospitals,—a conclusion to which some have thought that the late discussions on the subject by medical men necessarily lead. The facts ascertained certainly show very clearly that much is wanted in the proper construction and management of hospitals, to render them equally healthy with the habitations of those who are obliged to resort to them, for the safety of their lives and the restoration of their health.

There is yet too much to be said, however, in favour of hospitals for the sick and hurt to prevent any discussion such as that lately carried on, from lessening public opinion in support of such institutions. But we must all hope, that the facts and observations which have disclosed many defects will have a most beneficial effect in causing great improvements and ameliorations to be made in the construction of these buildings,—the defects of which are productive of the cause of those less successful results in medical and surgical practice which have been experienced, more than anything else. In short, the evils pointed out are remediable, for they may be lessened, counteracted, or entirely removed.

I need not, I presume, recall to your attention the great and important advantages derived from hospitals for the sick and hurt, further than to say, that it is not alone the patients who resort to them, the physicians and surgeons who practise in them, or the medical students who attend them, who benefit by them—the whole community, the public in general, and the public service of the nation, equally derive important advantages by the improvement in the "healing art" acquired in these institutions; and also by their lessening the extent to which contagious diseases may spread, if those first affected are not separated and removed from their neighbours, but which, in the case of the lower orders, can only be effected by a removal to hospitals.

On the other hand, if such hospitals, so long and firmly and so deservedly established in public estimation, by the great benefits they have conferred, were to be abandoned and swept away, a very great want would soon be felt, not only by the lower orders of the community, whose claims upon them are of the most urgent nature, but also by medical practitioners and their schools, ever zealously desirous for the advancement of their profession, both as a science and as an art, which can be best cultivated in the extensive field of practice presented to them in a large general hospital.

(7.) *Efficiency and Economy* in the construction of hospitals require particular attention from those who are concerned in forming plans for them. But efficiency is the first object. Economy is only secondary to it. The remarks I have already made relate chiefly to the efficiency of an hospital as depending on its construction. Economy can only be practised within certain limits, because experience has shown, that the wards of an hospital and the accessories now required, demand very extensive accommodation, which cannot be much lessened without injury to its efficiency.

## IV.—*Of the different forms of Hospitals.*

I come now to describe shortly the different forms of hospitals which have been erected in this and other countries, and to point out what appear to be the advantages and disadvantages of each. We may then be able to arrive at the conclusion as to the best form of a general hospital for the sick and hurt.

It will simplify my description of hospitals very much to classify them according to their internal construction.

Upon a wide and general survey, we find that all hospitals have been constructed on one or other of two different systems—the pavilion and corridor systems, or upon a combination of these two. I shall, therefore, arrange them into three classes.

(1.) *Class first.*—THE PAVILION SYSTEM.—In the first class I include all those hospitals consisting of a single line or block of building, or of several blocks connected together, having their wards occupying the whole breadth of the building. Wards of this form have been called "pavilion wards," or those constructed in the pavilion form; and the hospitals by which they are composed, as constructed on the pavilion system. [Sketches of pavilion wards and pavilion hospitals were here referred to.]

Each of these pavilions forms a separate hospital, possessing all the necessary requirements,—namely, wards of proper size and proportions, windows on both of their opposite sides, open fireplaces, and other accessories suitably connected with it; and the whole pavilion is capable of being shut off and isolated from the general building or hospital of which it forms a part.

This pavilion form of wards being now most generally approved of, many of the recently erected hospitals present to us examples of this pavilion system, including, in their details, all modern improvements.

As an example of a small county hospital constructed on this system, that of the Surrey County Hospital, Guildford (erected *in memoriam* of the late Prince Consort), is very complete in all its arrangements and details. But as it consists of only one ward for 26 male patients, and another for females, it can only be employed either as a medical or as a surgical hospital—the placing together of medical and surgical cases in the same ward being highly dangerous and improper. No county hospital, therefore, should consist of less than four wards, in order that the medical and surgical cases may be kept separate from each other.

The large pavilion hospitals, about which so much has been said of late, are constructed on a much grander scale, and are different in the arrangements of their pavilions and wards. They are ranged and connected together by passages or corridors at their lower floors, so as to form one large general hospital.

In form and principle the wards individually are the same in these as in the smaller hospitals. But when a number of pavilions form one large general hospital, they may be connected and arranged in several different ways, as those of the Hospital Lariboisière of Paris, Herbert Hospital, Woolwich, the new St Thomas's Hospital, London, and others.[1] The first of these is composed of ten pavilions, five of them being ranged on each side of a large court; St Thomas's Hospital consists of seven pavilions ranged in one row. Each pavilion of these hospitals, in which there are wards for patients, is of three floors in height above a basement floor, and, consequently, they are about 70 feet in height. Hence, when such pavilions are placed near to each other, as at a distance of only from 60 to 100 feet, they may act as a screen to one another, by preventing the free circulation of fresh air around them, and the sun's rays from falling upon them; and, besides, the intervening spaces or courts must be damp and ill-aired. I may add, that the pavilion form of wards is not new; but the arrangement of them into one large hospital is of modern date.[1] [Here enlarged plans were referred to as illustrating these remarks.]

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[1] See Appendix, No. III.

Every ward in the pavilions of St Thomas's Hospital is intended to be as complete and perfect as possible in every respect.[2] [Here an enlarged plan of one of these was referred to.] The main features of these pavilions and wards are:—

1. Complete separation from each other.
2. Each pavilion forms an hospital of itself, which may be shut off and isolated from the other parts of the hospital when required.
3. The wards have windows on both sides and open fireplaces.
4. Their form is most favourable for the free circulation of air and abundance of light.
5. They have the necessary accessories attached to them, though apart from the wards. These consist of consulting rooms, nurses' rooms, a ward kitchen, a small separation ward, lavatories, baths, and water-closets.
6. From the manner of these arrangements, the wards are very complete and easily superintended.

*Objections.*—Several objections have been made to this pavilion form of hospitals. Two of these only seem to call for any remark.

1*st*, It has been objected to as being more expensive than one large block of building. No doubt this must be so to a considerable extent; but, if possessed of superiority and advantages over other forms, are these not worth some greater cost?

2*d*, It has been alleged that this isolated and exposed form of wards is unsuited to the coldness of this climate, and would require additional means for heating them. This objection, however, has not been felt or complained of where wards of this form have long been in operation, as those in the old part of our Edinburgh Infirmary. In warm climates this objection could have no weight.

*General Remarks.*—If these pavilion hospitals, composed of wards, theoretically so excellent and complete, do not answer expectations as to their salubrity, there must be some fault existing as to the situation and relative position of the pavilions to each other, or in the regulation of their details when in operation. I must add, however, that for certain reasons they are still considered to be on their trial, and will require some longer time and experience to test their merits.

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[1] The Hospital Lariboisière was built to contain 600 patients, at a cost (it is said) of £400,000. Each ward, for 22 beds, is 115 feet long, 26 feet broad, and 15 feet in height, by which each patient will have 1900 cubic feet of air space.

[2] The new St Thomas's Hospital, London, is to contain 588 patients, at an estimated cost of £360,000. Each ward, for 28 beds, is 120 feet long, 28 feet broad, and 15 feet in height, by which each patient will have 1800 cubic feet air space.

But the most important question regarding them at present appears to me to consist of the proper and best arrangement of the pavilion blocks of which they are formed. In order to secure the benefit of the free circulation of air and sunshine, their position and distance from each other seem to me to be matters of the greatest importance; for it is always to be kept in view, that the best form of an hospital is that which secures most light and sunshine, fresh air and heat, to its inhabitants.

The improvements yet to be made on these hospitals, therefore, I am disposed to think, will chiefly consist in the most suitable and advantageous arrangement of their pavilions, and in their height being limited to one or two floors.

(2.) *Class second.*—Hospitals constructed on the CORRIDOR SYSTEM.—These form the second class of hospitals, to which I now beg to direct your attention.

These hospitals are so constructed that, in place of their wards occupying the whole breadth of the building, they form only one side of it, having on the other side a passage or corridor from which they enter. In some of these hospitals the corridor occupies only one side of the building, as in the great General Hospital of Hamburg; in others, the corridor occupies the centre of the building, as in several hospitals which could be named, there being wards on each side of it. This last form or arrangement of an hospital appears to be the most objectionable of all. The wards, having windows only on one side of them, cannot have that free ventilation which is necessary, and the corridors in their centre can neither possess that sufficient light nor fresh air which they ought to have. In these wards, the invigorating currents of fresh air, which pass through wards having windows on opposite sides of them, cannot be obtained, the foul air stagnates, the wards get unhealthy, and alterations are required to improve them, such as the removal of partitions, or making apertures through them for the admission of fresh air; but these alterations are seldom satisfactory, and have been found only to lessen the evils and disadvantages in these hospipitals caused by their construction. This form, or corridor system of hospitals, is now generally considered to be so objectionable that the pavilion system is preferred. I regret to add, that I consider it beyond dispute that the corridor hospitals are the most unhealthy, notwithstanding extra appliances, care, and attention for the health of their inmates; but if small wards are considered best, corridor hospitals admit of these, as in the Hamburg Hospital. Most of the lunatic asylums in this country are on this system.

(3.) *Class third.*—Hospitals constructed on the PAVILION AND CORRIDOR SYSTEMS COMBINED form a third class, into which I have arranged them. St George's Hospital, London, is an example of this form of an hospital. In this the central part has the form of a corridor hospital, with wards on both sides of the corridor, while the wings attached to it consist of wards of a pavilion form. [Enlarged plans of the Hamburg and St George's Hospitals were here referred to.] Many existing hospitals of old date have been constructed in this form, which, in several respects, is simple and good; but several of their wards must be defective in the means of ventilation required. Nevertheless, many of them have proved very efficient in practice, and still continue to do so.

*Of buildings surrounding a central court.*—To the different forms of hospitals which I have described, I have to add that hospital buildings surrounding a court or enclosure have been considered to be unfavourably situated for the circulation of fresh air around them and into their wards. But when the central court is large, the buildings not too lofty, and situated with sufficient open space outside, such an arrangement should not be objectionable.

### V.—*Accommodation for the Officials of Hospitals.*

After all that I have said regarding the situation, form, and other arrangements of the wards of hospitals, I must remark that, although very important, these are not the only objects which require particular attention in the construction of hospitals. The "administration," or officials of different classes, who conduct their operation and management, require ample and extensive accommodation; and many accessories require the same; so that when I enumerate the chief of these, and direct your attention to the proportion of the buildings of St Thomas's Hospital, and of the Lariboisière and other hospitals, which they occupy, you cannot fail to observe their importance, and the great extent of space they necessarily occupy.

To provide proper and suitable accommodation for the board of management; rooms for attending physicians and surgeons; apartments for the resident medical officers; a hall, library, and museum for students; waiting rooms for patients; accident rooms; theatres for lectures and operations, together with proper accommodation for the apothecary, the house steward, the matron, the nurses, and other servants of the institution; as also kitchen, laundry, storerooms, and others, it must be evident, not only that much space is required, but also much skill and attention, to place each of these adjuncts in the most convenient and appropriate position for their different occupants and purposes, which can only be done by the combined knowledge and skill of the medical and surgical practitioners and architects.

### VI.—*Conclusion.*

Keeping in view those principles which I have endeavoured to represent, as established by the experience and general opinions of the medical profession, for the proper construction of hospitals, I shall conclude by suggesting a Plan (as an example in illustration of the subject), by which these principles may be most advantageously combined and carried into effect. In doing so, I shall keep within those bounds which I described as the province of medical men in this matter.

Assuming that the pavilion system is to be adopted as the best, I have here sketched out a plan or diagram of the first floor of an hospital, which embraces all that is required, and what I consider an improved and less objectionable arrangement of its pavilions than that of the great hospitals I have referred to.

(*a.*) This diagram is intended to represent a plan for a large general hospital on the pavilion principle, for about 400 patients.[1]

(*b.*) It consists of two distinct and separate buildings for the medical and surgical departments. These, again, are each separated into two divisions for male and female patients.

(*c.*) For efficiency and economy, each division consists of a double pavilion; these being after the model of those of St Thomas's Hospital.

(*d.*) These pavilions are intended to be of two floors only above the basement floor, and of medium size.

(*e.*) The pavilions are connected by corridors of only one floor in height. In connexion with these corridors also are theatres, consulting rooms, waiting rooms, and others, for the resident medical officers, apart from the wards.

(*f.*) Each of the four double pavilions contains four wards, each to contain from 22 to 24 patients.[2] And they each possess the accessories required—consulting room, kitchen, small ward, etc.

(*g.*) Each pavilion can be isolated, or shut off, from the other parts of the hospital.

(*h.*) Smaller special wards, day rooms, and ophthalmic wards are intended to be added. The whole to contain 400 patients. The basement floor would be occupied by the departments of the apothecary, matron, and others.

(*i.*) The central court is large, being above 200 feet square, and free ventilation is assisted by the entrance openings at each side.

(*j.*) The pavilions are so placed, and at such a distance from each other, that they are not likely to screen each other by intercepting air and sunshine. And the corridors, being only one floor in height, cannot injuriously confine the air within the central court. The pavilions are intended to stand in a line from east to west, by which one side of them will be exposed to the south sunshine.[3]

*General Remarks.*—If the expected excellence of pavilion hospitals is proved and established, as seems most probable, I contemplate that they will cause a complete revolution in Scotland in the planning and construction of hospitals, asylums, and poorhouses; so that the example of England and other places[4] may be followed, as I trust it will be, with very beneficial results. But let those who

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[1] See Plate I.

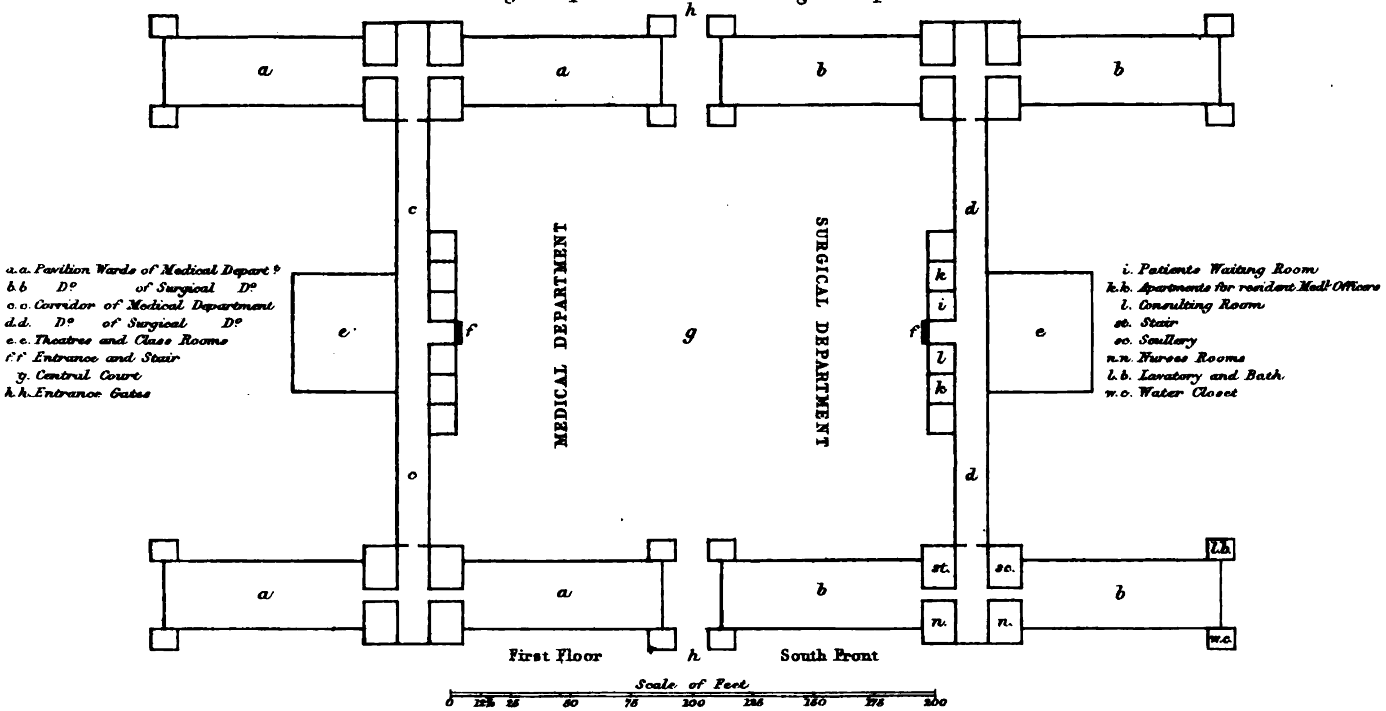
[2] Each division will contain about 100 patients.

[3] See Plate II.

[4] France, Algeria, America, etc.

Plate I.

## SKETCH FOR A GENERAL HOSPITAL ON PAVILION SYSTEM
Consisting of Separate Medical and Surgical Departments.



J. Bartholomew, Edin.r

adopt this form of hospitals see to it that it has fair play—that the pavilion blocks are not too lofty—that they are placed on a proper site—that they are properly arranged, and not too close together; for if these important requisites are not judiciously fulfilled, the advantages to be derived from it may be frustrated, and not obtained to the extent expected. If the premises I have laid down are correct, any deviation from them must be wrong; and, if to a great extent, must end in a complete failure.

In the remarks I have had the honour to submit to you on the different forms of hospitals, I have avoided as much as possible to occupy your time with too many details; and have endeavoured to confine my observations to the more prominent and essential points required in the construction of these buildings, at once designed to combine efficiency, as regards the treatment of the patients,—economy as regards the public, who generously maintain these institutions,—and also as the form best adapted for medical and surgical experience, as regards the cultivation of the healing art.

A Fever hospital, a Maternity hospital, a Lock hospital, and a Lunatic hospital, should each have a separate locality, and be situated at a distance from a General hospital for the sick and hurt. Had time permitted, I might have made some additional remarks on each of these, with reference to the special construction they severally require. But I have already occupied your attention too long.

And now, if the remarks I have made for your consideration on this important subject tend, in any measure, to solve the great problem of combining and including what experience and science have shown to be desirable and necessary in the construction of hospitals for the relief of the sick and hurt—as affording the best means for the care and curative treatment of the suffering and destitute portion of our fellow-creatures—imparting to them, by its ample provisions for their welfare, confidence, hope, and encouragement amidst their troubles, giving health to the sick, sight to the blind, and making the lame to walk, and thus restoring many heads of families to their homes, to resume that parental care and support to those depending on them, of which they had been deprived by sickness or accident, and thereby, the Hospital, by the good it confers, giving the best assurance to the benevolent and generous of the community, that their charity is most beneficially and faithfully applied to the purposes intended, my object will be attained, and my most anxious desire amply gratified.

## APPENDIX, NO. I., PLATE II.

*Sunshine and Light.*—There is a prevailing idea among those who are concerned in the construction of hospitals, that, by placing the pavilions with their long axis north and south, their wards will receive more sunshine into them, than if placed in the opposite direction, from east to west. This I consider to be a great mistake, as the accompanying diagram plainly shows. (Plate II.)

Figure 1.—A represents a pavilion within a circle indicating the position of the sun at different hours of the day, as marked upon it. The narrow ends of this pavilion are placed north and south. At 6 o'clock in the morning, if the sun is up, its rays strike directly on the long side of the pavilion. But from that hour till 12 o'clock, the rays fall less and less directly, and more and more obliquely, upon it, till at length they strike upon the south end of it at that hour, without getting any admittance. After 12 o'clock, the sun's rays again shine upon the side of the pavilion, obliquely at first, but they gradually do so more and more directly, till 6 o'clock.

In this progress of the sunshine, it is obvious that the rays abound most in the wards at 6 o'clock A.M., and become of less and less extent till mid-day, when none of them enter the wards at all; while from 9 till 3 o'clock, the entering rays are oblique and few in number.

Figure 2.—B, in the centre of the circle, represents the pavilion placed with its long axis from east to west. Here the rays of the sun are represented as falling most copiously and directly upon the side of the pavilion at 12 o'clock, when its rays are strongest. It will also be observed that the rays at 6 o'clock A.M. fall directly on the end of the pavilion, at a time when they are much less effective; but they gradually increase in directness and extent upon the side of the pavilion till 12 o'clock, when the sun's rays are strongest. They then gradually decrease as the evening advances.

When we consider, further, that when the days become short for several months in winter, and the sun only shines from 8 or 9 o'clock till 4 or 5, the oblique rays falling upon the pavilion between these hours, as shown in Fig. 1, are not to be compared in efficiency with those more direct rays upon the side of the pavilion during these hours, as shown in Fig. 2.

It, therefore, seems to be quite a fallacy to suppose, and adopt the opinion as correct, that the pavilions would derive any greater advantage from the sun by being placed with their ends to the north and south, than if placed east and west, but rather much the contrary.

## Appendix, No. II.

*Ventilation.*—The pavilion system for wards and hospitals is theoretically approved of by all; but in some cases, where it has been carried into practice in a very complete manner, there has been disappointment from apparent failure as to salubrity. To what is this to be ascribed? The chief cause alleged for this is the want of sufficient ventilation. There seems to have been too much dependence placed in the system of artificial ventilation employed, and expected to have been secured, by the complicated constructions and machinery introduced into the buildings. Where this has been done, it must have considerable influence; because the successful operation of such constructions requires the absence

**Plate II.**

**Figure I.**

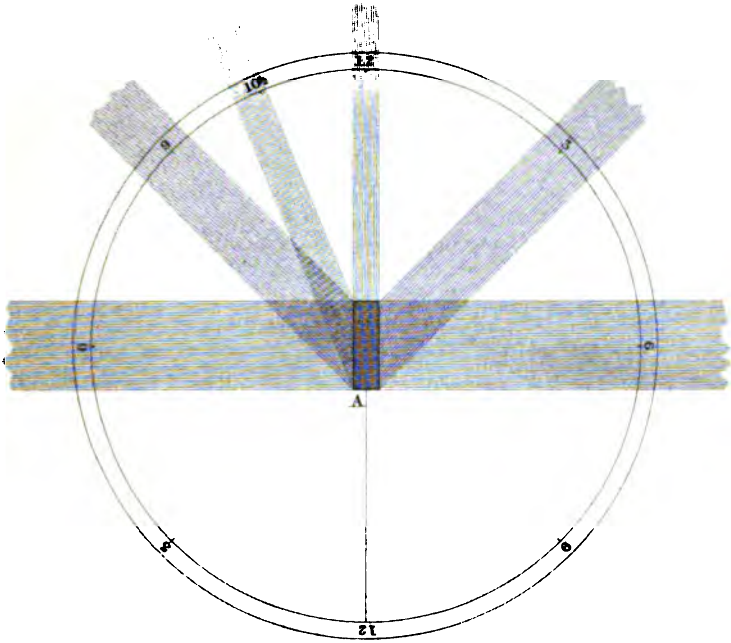
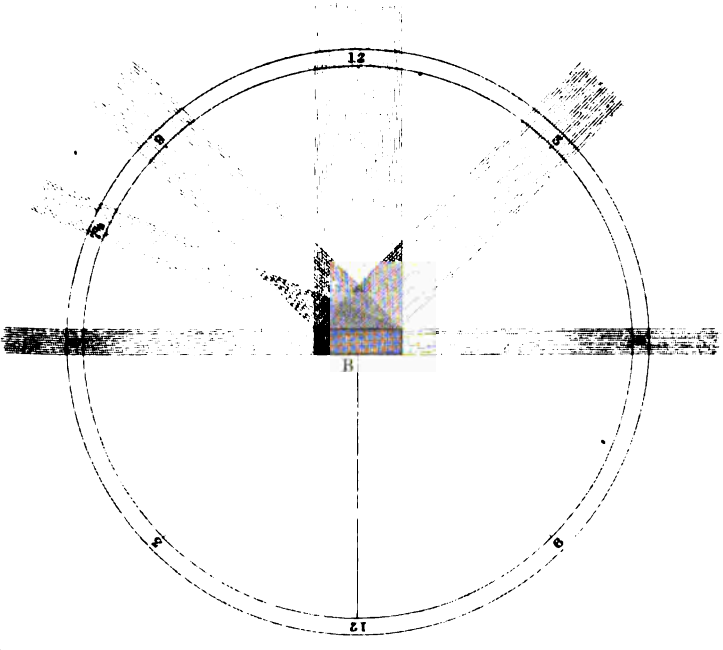


**Figure 2.**



of open fires and windows. So that all natural ventilation for supplying fresh, and removing foul, air, has to be prevented; and its place supplied by heated, and probably polluted, air forced into the wards by machinery, which cannot be expected to work with regularity, or be always properly managed by attendants. Besides, such constructions are expensive to erect and maintain in operation; and general experience has shown that they are not to be relied on. The influence of this mode of ventilation, therefore, cannot fail to be detrimental and disadvantageous to the health of those confined in hospitals so constructed. Hence, artificial ventilation should not be trusted to, in preference to natural ventilation, which has these several advantages:—(1.) It always supplies pure air. (2.) It can always be established without any additional expense in the construction of an hospital. (3.) It is not expensive in operation. (4.) It is easily regulated. (5.) It seems to be that mode intended by Providence for the continued supply of fresh air to man and beast—to be used and regulated by properly-constructed buildings, and not counteracted, obstructed, prevented, or excluded, in order to be superseded by complex machinery and impure air; and, therefore, artificial means of ventilation should only be employed as auxiliaries to natural ventilation.

## APPENDIX, No. III.

*Position of Pavilions.*—Another cause of defective ventilation consists in the arrangement and position of the pavilions composing large hospitals. When they are ranged close together, and parallel to each other, with too little intervening space,[1] the free circulation of fresh air around them is prevented. The wind may beat strong upon the side of the pavilion opposed to it, but this one shelters and screens the others from its influence. The foul air, also, of one pavilion may be blown into the open windows of that next to it; and the stagnant, and probably damp, air of the intervening court may find its way into the wards. These are evils which may be prevented by the pavilions being placed in long straight lines, at a sufficient distance from each other, as in the plan proposed in the preceding pages. These lines should be placed from east to west; for, in this position, the prevailing winds in this country would strike them on their ends, and, passing along their sides, would supply fresh air to them in a gentle manner, and thus the pavilions would be little affected by storms.

The objectionable position of the pavilions of hospitals is exemplified in that of the Herbert Hospital, Woolwich (Fig. 2, page 880), in which the pavilions consist of a basement and two ward floors above it, making the height 54 feet, and are only 64 feet apart from each other. Their extremities are also placed nearly north and south; being altogether in the most unfavourable position either for sun or air.

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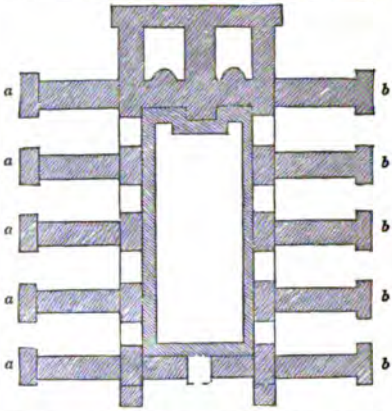
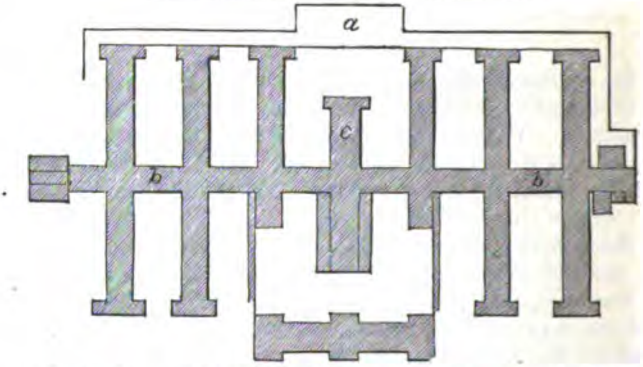
[1] See page 880, Fig. 1.

FIG. 1.—HOSPITAL LARIBOISIÈRE, PARIS.



The position of the pavilions (*a*, *b*) of the Lariboisière Hospital, Paris, is equally objectionable, as seen in the above figure. See page 872.

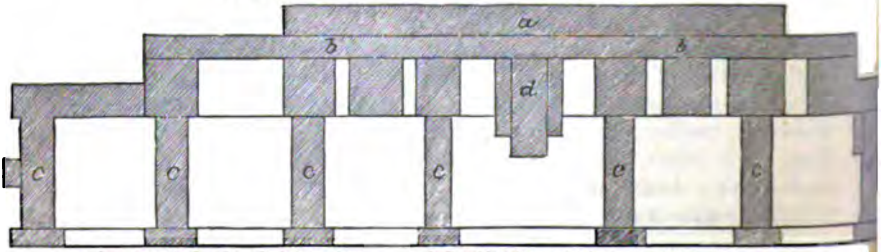
FIG. 2.—HERBERT HOSPITAL, WOOLWICH.



*a*, Promenade. *b*, Corridor by which the pavilions are connected and entered.

In this instance the pavilions are too close together, and stand in a wrong direction, as already stated.

FIG. 3.—NEW ST THOMAS'S HOSPITAL, LONDON.



*a*, Apartments for the administration. *b*, Corridor from which the pavilions are entered. *c*, The pavilions.

The foregoing figure, showing the position of the pavilions of the new St Thomas's Hospital, London, also illustrates the remarks above made against this arrangement.

The pavilions of this hospital are about 70 feet in height, consisting of a basement floor, three ward floors, and attics, and the distance between the pavilion blocks is only 125 feet.

Considerable disappointment has already been felt in regard to the salubrity of several of the hospitals erected in this manner; and if the above statement as to the alleged causes of this is correct, it follows that some better arrangement of the pavilions is necessary, and should be adopted; for any mistake in this matter could not easily be remedied.

## ARTICLE II.—*Observations on Amputation at the Hip-Joint.* By THOMAS ANNANDALE, F.R.S.E., Surgeon to the Edinburgh Royal Infirmary.

ALTHOUGH the use of chloroform and the efficient compression of the abdominal aorta have tended to diminish the immediate risks of amputation at the hip-joint, the mortality of this operation is still very great. This mortality is principally due to the severe shock which results from the infliction of so large a wound, and the removal of so considerable a portion of the body. If the patient survives the first period of shock, the accidents, irritation, and exhaustion attendant upon the healing of the large wound also produce, in the majority of cases, a fatal result.

I think it is justifiable to consider, that the immediate shock to the system, caused during the infliction of this and other large wounds, is much diminished, if not prevented, by having the patient completely anæsthetized; and there can be no doubt also that, with the assistance of the abdominal clamp, amputation at the hip-joint may be performed, with the loss of only two or three ounces of blood.[1] Surgeon S. U. Gross[2] says, "The ratio of mortality in all hip-joint amputations has certainly diminished since the introduction of chloroform, and I think we possess in it a powerful agent against shock, which in former days was so fruitful a source of mortality." He says, further, "Hæmorrhage, as a source of danger, is now robbed of its terrors by the employment of the abdominal tourniquet for compressing the aorta." Mr Gamgee,[3] in his interesting account of a case of amputation at the hip-joint, remarks—"But so effectual was the aortic tourniquet that the great arterial mouths were left open and dry, and the operation was concluded as deliberately, and with as little hæmorrhage, as an ordinary amputation."

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[1] The total loss of blood may be still further diminished in many cases by carefully bandaging the limb, and raising it above the level of the trunk for a few hours before the operation. This proceeding is especially useful in cases of large and vascular tumours.

[2] United States Report on Amputations at the Hip-Joint. Circular No. 7.

[3] Amputation at the Hip-Joint. By J. Sampson Gamgee.

Mr Erichsen,[1] in his remarks upon this amputation, says, "The employment of Lister's aorta compressor will probably still further reduce the mortality, by lessening the loss of blood, and thus proportionately diminishing those risks of low secondary disease that are induced by serious hæmorrhage." Professor Miller[2] remarks, "The operation is certainly deprived of its horror and danger, to a great extent, by the use of chloroform, and by the employment of the abdominal aorta compressor."

Seeing, however, that even with these improvements the mortality of amputation at the hip-joint is so great, it becomes an important question for the practical surgeon to inquire whether this mortality cannot be still further diminished?

In considering this question, my remarks apply only to those cases in which it is necessary to remove the entire femur by disarticulation, and I would ask—

First, *Can the cut surface made in this amputation be in any way diminished so as to lessen the general shock?*

In some cases requiring this formidable operation, the surgeon has no choice, but must cut his flap or flaps from the most convenient aspect of the limb, and perform disarticulation. In other cases the operation may be performed according to rule, that is, by antero-posterior or lateral flaps, for these are the methods now usually recommended when there is a choice of proceeding. In a few remaining cases the soft textures may be divided at a lower level (through or immediately below the trochanters), and the head of the bone then dissected out or disarticulated. It might reasonably be supposed that the last of these methods would be the least dangerous—(1st), Because the soft textures are divided further from the trunk; and (2d), Because the wound is apparently smaller.

Erichsen[3] in his observations on cancer of bone, remarks,—"When the lower part of the thigh-bone, however, is involved, amputation through the trochanters may sometimes be substituted for disarticulation at the hip-joint, the latter operation being so formidable and so fatal that the surgeon may not think it advisable to subject the patient to so serious a risk; or amputation might be performed through the trochanters, and then the head of the bone extirpated from the acetabulum. In this way the severity of the operation and the extent of incised surface would be lessened, whilst the whole of the diseased bone would be removed."

Surgeon R. B. Bontecou,[4] in his evidence on amputation at the hip-joint, says, "I should, however, make the operation by circular incision of the upper third, soft parts permitting, and enucleate the bone by incision, carried up the outside of the limb to the trochanters, believing that the hæmorrhage and shock to the system would be less than by the usual methods."

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[1] Science and Art of Surgery, vol. i. p. 61.

[2] Principles of Surgery, vol. ii. p. 1376.

[3] *Loc. cit.*, vol. ii. p. 151. [4] *Loc. cit.*, Circular No. 7.

Acting on this principle, I have during the last twelve months operated upon two cases. In the one case the amputation was performed according to Surgeon Bontecou's plan for long-standing ulceration of the leg and thigh, and proved fatal on the fifth day. In the other, the operation was performed by cutting a short anterior and a short posterior flap immediately below the trochanters, sawing through the bone, and then dissecting out the head of the femur after the vessels had been secured. The disease in this case was a cancerous tumour affecting the lower third of the femur, and the patient lived until the fifth week, when he was suddenly seized with difficulty of breathing, and died forty hours after. The post-mortem examination of this patient showed a partially decolorized clot in the pulmonary artery; the surfaces of the flaps were firmly united, but there was an abscess in the acetabulum; there were no morbid traces of pyæmia.

Being anxious to determine the relative extent of cut surface in these methods of amputating at the hip-joint, I recently made the following observations:—

In an adult male subject, I performed amputation of the right leg at the hip-joint by a long anterior and short posterior flap, and removed the left limb by cutting a short anterior and a short posterior flap immediately below the trochanters, sawing through the trochanters, and then dissecting out the head of the bone. Having then carefully arranged the flaps, and filled up the acetabular cavities, I took a cast of the whole cut surface in each stump. In order to obtain a cast of the surface of a circular amputation, I made a circular division of the soft textures in the left stump opposite a point immediately below the trochanters, and at the same time carried an incision upwards from the outer edge of the wound to the top of the great trochanter, as suggested by Surgeon Bontecou. A cast of the surface of this stump was also taken. Three casts were thus obtained, which, for the sake of convenience, may be named 1, 2, 3. On first looking at these casts, it appeared that the two obtained from the stumps made by a low division of the soft parts had a much less extensive cut surface than the other; but in order to determine this, I asked Messrs Blyth and Cunningham, the eminent engineers, to measure these surfaces for me.[1] The following is the report of their careful measurements:—

Cast 1, taken from amputation at the hip-joint by a long anterior and short posterior flap, measured 70·54 square inches.

Cast 2, taken from amputation immediately below the trochanters by a short anterior and short posterior flap, measured 80·75 square inches.

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[1] I will be glad to show these casts to any one interested in the subject.

Cast 3, taken from amputation immediately below the trochanters by circular incision, measured 76 square inches.

This question must, therefore, I think, be answered in the negative; for, if my observations be correct, they prove that the extent of cut surface in the amputation usually performed is considerably less than that resulting from the other methods referred to; and I do not think that the mere division of the soft textures at a lower level can compensate for the larger cut surface which such a proceeding causes.

Second, *Can any special treatment of the wound be adopted, so as to influence or lessen the irritation and suppuration during the progress of its healing?*

This question may, I think, be confidently answered in the affirmative.

The valuable observations of Mr Lister, and the recorded experience of other surgeons, have undoubtedly proved that the suppuration and local and general irritation connected with the largest wounds, are entirely prevented, or very much lessened, by the careful employment of carbolic acid or other antiseptic means.[1]

An additional assistance in preventing or lessening the local irritation is the use of the antiseptic ligature (silk or gut) to secure the divided vessels. In my cases of amputation at the hip, the femoral artery and vein and the smaller vessels were secured with complete success in this way (with gut), and I have, during the last twelve months, used the same ligature successfully in three cases of amputation through the thigh, and also in many other operation wounds.[2]

It is to these means, and to a careful attention to the diet and general condition of the patient, that, I think, we must trust, in order to diminish the mortality following amputation at the hip-joint and the infliction of other large wounds. My own experience of the antiseptic treatment leads me to express a strong opinion that, with care, it will prove successful in bringing about this happy result.

My conclusions in connexion with the subject of amputation at the hip-joint consequently are—

(1.) That chloroform or ether, and the abdominal compressor, are valuable aids in diminishing the immediate risks of this operation.

(2.) That the shock resulting from this operation is not influenced by any low division of the soft textures.

(3.) That the careful antiseptic treatment of the wound, and the use of the antiseptic ligature, are the means most likely to diminish the secondary risks of this operation.

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[1] See the Report of a Case of Amputation at the Hip-Joint, by Mr de Morgan, in British Medical Journal, 12th February 1870.

[2] In the case of the smaller vessels, I find that torsion answers admirably, but I prefer securing the larger ones with the antiseptic ligature, its ends being cut close off.

## ARTICLE III.—*An Aid to Parturition, and to the Treatment of Displacements of the Uterus, by a new Mechanical Appliance.* By PROTHEROE SMITH, M.D., M.R.C.P., Physician to the Hospital for Women, London.

(*Communicated to the Obstetrical Society of Edinburgh, 8th December* 1869.)

WHAT makes the contrast between the easy and rapid childbirth of the savage state, and the painful and lingering process in civilized life?

In answering this inquiry, I shall endeavour to elucidate the chief reason of this obvious difference, and thence to show what gives the advantage to the savage. I shall then endeavour to prove that a force, which exists in the one, and is deficient in the other, explains to a great extent the dissimilarity of the two instances. I hope, then, to propose a remedy for the defect, and to give my experience of its efficacy. The same question applied to uterine ailments is capable of a similar explanation, and its solution will, I venture to say, elucidate an important cause of the frequency of diseases of women in civilized life.

Man being erect, it necessarily follows that the structure of the pelvis and abdominal muscles must be such as to retain the viscera in their proper position, and to make the necessary expulsive efforts, which defecation and parturition require, with impunity.

The spinal column thrown into a series of waves, not only lessens shock from concussion, but by its lumbar curve the plane of the pelvis is rendered oblique, constituting that graceful form, which when absent is artificially supplied by a pad or bustle, and which is so designed as to support the abdominal viscera on the pubes, aided by the abdominal muscular walls and spine, and to retain them within the pelvis. But when the plane of the pelvis becomes horizontal by the obliteration of the lumbo-spinal curvature, or, as it is called, the Grecian bend, the abdominal viscera tend to prolapse towards or beyond its outlet, and to displace the pelvic contents.

This lumbar curve being absent at the extremes of life, the infant is found to be straight-backed, whilst the common evidence of old age is obliteration of the double antero-posterior curves, going on to the bowed back and drooping form of those who are well stricken in years. But the deficiency of the natural spinal curves is more or less known in early adult life, it may be from disease of the bodies of the vertebræ, with which we have at present nothing to do, but its chief cause may be found to be defective muscular power, which in its cause and effect forms the chief subject of the present paper.

Unlike the ancients, who suspended their garments from the shoulders, the usages of modern society impose upon females, more or less from the period of birth, the habit of wearing a cincture round the waist. From this are suspended all the clothes, which by their weight keep up considerable pressure on the thoracic, lumbar, spinal, and abdominal muscles; and this persistent pressure often leads to degeneration of their tissue.

Thus the muscles used in diaphragmatic respiration, and for fixing the chest in forced efforts of expulsion, as well as those of the spine and abdomen, are enfeebled by absorption or degeneration of muscular fibre, and so their functions are impaired.

This alteration in the muscular tissue is well known to all who have witnessed the result of tight-lacing in the dead-house, in the muscular atrophy which marks such cases, and attenuates the abdominal walls. The consequent obliteration of the lumbar curve, and the levelling of the plane of the pelvis, destroy the natural lines of the female figure, and open the door to all those maladies which result from the above-named deformity of the spinal column and of the pelvic outlet.

Two great canals are lodged within the pelvis, viz., the alimentary and the generative. For the expulsion of the contents of the one in the act of defecation, there is necessarily—

1. The involuntary contractile force of the bowels; and
2. The voluntary force supplied by the spino-abdominal muscles.

In this act the direction of the pelvis is altered, so as to facilitate the passage of the fæces by means of an inclined plane, over which they are readily expelled. In the process of parturition, there is also—

1. The involuntary contractile force of the uterus.
2. The voluntary power of the spino-abdominal muscles.

In labour, the expulsion of the child through the pelvic canal is protracted, in like manner, for lack of muscular power, and the process at times seems almost left to the unaided force of the womb. Hence this organ is overtaxed, and sometimes fails in its efforts till helped by instrumental traction.

Of the two uses now claimed for the pelvic band, the first is to supply artificially that force which may be deteriorated by dress, or suspended by anæsthetics, and by so doing to aid the overtaxed uterus, by curtailing the period, and so to lessen the risks of ordinary labour; the second to aid the treatment of uterine maladies.

I will now briefly describe the instrument, showing wherein it differs from, and in what way I believe it is superior to, any that have hitherto been brought before the profession.

The advantages of it, when used as an aid to parturition, are—

1. That a fixed point is secured for the exercise of mechanical force by means of, as it were, an artificial skeleton, the immobility of which is secured by two pads, one sacral, the other pubic, retained in their respective places by lateral springs, which embrace the pelvis below the crista ilii.

2. There having been gained a fixed point, attached to the sacral pad are two vertical levers, and one horizontal; the former (vertical springs) end each in a costal spring, embracing the thorax, and united by a sternal pad, so arranged as not to incommode the chest

in respiration; the latter or horizontal is a powerful spring buckled to a belt closely adjusted to the abdominal walls, and connected below to the pelvic band by elastic rings. It is thus constructed so as to act in the manner of the abdominal muscles. During the parturient pains this belt is readily tightened at will to assist or supply the place of the voluntary muscular contractions, and is as readily relaxed in the intervals of pain. When required, it may be made to maintain a steady uniform pressure as post partum is often needed. This force reacting upon the sacral pad affords to the patient that support in the lumbar region which is so much called for during labour, and which is usually supplied by the hands of the nurse.

In the catalogue and report of instruments published by the Obstetrical Society in 1867, there is at page 5 an ingenious contrivance of Dr Woodward of Worcester, which seems to answer the purpose last referred to, and which he accordingly named an Obstetrical Back-Supporter. When the power of the abdominal muscles has been impaired by the constraint of dress, or rendered deficient under the influence of anæsthetics, the instrument I now exhibit supplies artificially a force which simulates that of the abdominal muscles when in a normal state. In this way, I have found it of the greatest value in cases of anteversion of the gravid uterus at the period of parturition. Only a few weeks since I quickly and easily rectified the abnormal position in a patient I have attended with four children. The ease and rapidity of birth offered a great contrast to her former labours, and the patient appeared herself to be better than on any former occasion. Her subsequent "getting up" was satisfactory in every respect, and she has regained her strength in a manner so superior to that which has happened in her other confinements as to elicit voluntary expressions of satisfaction and surprise from herself and friends.

For the mechanical treatment of uterine ailments, this instrument acts in the following way:—The pubic and sacral pads fixed by the lateral connecting springs tend, by their mutual reaction, to alter the plane of the pelvis, if too horizontal, to one more oblique, and therefore more natural. The costal springs and sternal pad, attached to the end of the perpendicular springs, help still further to increase the lumbar pressure, to throw the sacrum backwards, and, at the same time, to fix the whole apparatus. From the pubic pad, and attached to it by a movable rackwork, is a curved steel spring, which, bent to the shape of the vagina, is passed into it, whilst it is made to carry at its extremities any form of pessary or support that may be required.

The advantage of a uterine support adjusted by these means is, that its *point d'appui* being the pelvis itself, in ordinary movements of the body it is not liable to displacement.

Without attempting here to enter into the pathology of misplacements of the womb further than by referring to the cause already mentioned—viz., that of a too horizontal position of the pelvis—I would state that, in order to rectify this, the prone position of the patient for a longer or shorter time has often been adopted by practitioners; and this mode of treatment was strongly advocated by my late friend and former colleague, Dr Rigby. To counteract such derangements, I have made many experiments, which have at length resulted in the production of this instrument.

A stand-point is thus obtained, whereby force can be gradually applied to the lumbar region, so as to alter the position of the pelvis from the horizontal to a more oblique plane. The same advantage is thus gained by the pelvic band without the confinement and constraint of the prone position, and of many other ingenious appliances, which nevertheless are much more complicated and irksome to be borne.

The principal feature of the pelvic band is that it forms an artificial skeleton, and so supplies all the chief *points d'appui* by which the complicated muscular system engaged in the acts of expulsion harmonize and effect their purpose; and it is so one with the pelvis itself as to offer a stability and comfort that no appliance depending chiefly on abdominal, and therefore unstable, pressure can possibly afford.

Enough, I trust, has now been said to warrant the hope that members of our profession will not be wanting to test its merits. Should this instrument obtain the approval of the Obstetrical Society of Edinburgh—should it prove to be, as I hope, an aid to midwifery, by lessening the pains and perils of childbirth, as well as by curtailing the often protracted attendance of the accoucheur—and should it likewise tend to facilitate the treatment of uterine dislocations,—my object in bringing the subject before the Society will be fully gained.

## ARTICLE IV.—*Remarks on Pelvic Peritonitis and Pelvic Cellulitis, with Illustrative Cases.* By LAUCHLAN AITKEN, M.D.

RATHER more than a year ago there appeared from the pen of a well-known gynækologist of this city a very able monograph on the two forms of pelvic inflammation whose names head this article; and it cannot have escaped the recollection of the reader that Dr M. Duncan, adopting the nomenclature first proposed by Virchow, has used different terms on his title-page[1] than those older appellations I still propose to retain. Under these circumstances I feel compelled at least to attempt to justify my preference for the original names; and I trust to be able to show that they are preferable to, and less confusing than, any others that have as yet been proposed, even though we cannot consider them absolutely perfect.

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[1] A Practical Treatise on Perimetritis and Parametritis (Edin. 1869).

Passing over, then, such terms as *periuterine cellulitis* or *phlegmons périutérins* as bad compounds; others, as *inflammation of the broad ligaments*, as too limited in meaning; and others, again, as *engorgement périutérin*, as only indicating one of the stages of the affection,—I shall endeavour as succinctly as possible to state my reasons for preferring the older names to those proposed by Virchow.

*1st.* The two Greek prepositions, *peri* and *para*, are employed somewhat arbitrarily to indicate inflammatory processes which are essentially distinct. I say arbitrarily, because I am not aware that *para* has been generally employed in the form of a compound to express inflammation of the cellular tissue elsewhere.[1] By those who remember that the cellular tissue not only separates the serous membrane from the uterus at that part where the cervix and body of the organ meet, but is even abundant there,[2] perimetritis might readily be taken to indicate one of the varieties, though indeed not a very common one, of pelvic cellulitis—a variety, in fact, for which the term perimetric cellulitis has been proposed. It is self-evident how much confusion might arise as to the real nature of a disease simply by the change of one letter of the alphabet.

*2d.* The term *perimetritis*, even if employed only to indicate inflammation in the peritoneum, could not well be used if that inflammation were confined to the serous membrane round the tubes or ovaries. For such varieties of pelvic peritonitis we should require to adopt the terms peri-salpingitis and peri-oophoritis; thus, in my opinion, unnecessarily encumbering our nomenclature. The truth is, that serous inflammation in the pelvis is rarely, if ever, confined to the peritoneal covering of one organ; and, besides, we have no means of diagnosing it, when it is. In those facts we find other contra-indications to the employment of a term so restricted in its meaning as perimetritis.

*3d.* The preposition *para* means, by the side of, and to most Greek scholars a word like parametritis would indicate too close a proximity to the uterus to be used with correctness in a case, for example, of puerperal cellulitis[3] in one or other of the iliac fossæ, a difficulty which the term pelvic cellulitis to a great extent removes. But besides, the preposition *para*, according to our best Greek authorities, such as Donaldson and Jelf, is never used to indicate proximity to a person or thing, except with the idea of motion towards or by the side of that person or thing.

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[1] Paranephritis is used by Dunglinson, for instance, to express Inflammation of the Suprarenal Capsules.

[2] Aran, Leçons Cliniques sur les Maladies de l'Utérus.

[3] I am perfectly well aware that Dr Duncan denies the extension of a mere cellulitis etiologically parametric, as he would say, to the iliac fossa. This is a point which can only be settled by further inquiry, and especially by autopsic demonstration. I can only say that I imagine I have seen a case of iliac cellulitis ending in abscess from an injury to the uterus without any matter having formed in the true pelvis; and a case adduced by Dr Duncan himself, at p. 155 of his work on Perimetritis and Parametritis, seems to confute his theory. (See also Bernutz and Goupil, Clinical Memoirs on the Diseases of Women, translated for the New Sydenham Society by Dr Meadows, vol. ii. p. 146.)

*4th.* The terms pelvic cellulitis and pelvic peritonitis show to any one, at a glance, the nature of the affection, whether it is inflammation in the cellular tissue or serous membrane of the pelvis, and do not limit the seat of the disease to the immediate neighbourhood of the uterus, as I maintain the words perimetritis and parametritis must do. If we keep in remembrance that the terms pelvic cellulitis and peritonitis are almost invariably employed by gynækologists when the inflammation in the pelvis arises from some kind of disease of, or injury to, the uterus and its appendages, I hold that, at least in the present phase of our knowledge of the affections, those names can produce no confusion whatever in the mind of any one who adopts them, but must necessarily assist him in rapidly forming a conclusion as to the seat and pathology of the disease.

Up to a very few years ago the pathology of pelvic inflammation was only partially elucidated, or had been in many cases erroneously interpreted. Some facts apparently well established, and, at any rate, supported by the weight of great names,[1] pointed to the cellular tissue as the seat of those inflammatory processes which were so frequently known to follow the act of parturition, and even to occur in the non-puerperal female from many and varying causes. But as our knowledge of pathology advanced, it came to be recognised that many of those cases which had been regarded as purely extra-peritoneal were in reality affections of the serous membrane itself and the consequences arising therefrom, and that, in fact, we had to deal with two perfectly separate and distinct diseases, which, indeed, might complicate each other, but of which, strange to say, the one first carefully described[2] was by far the most rare, and by much the least important.

It would be unjust to one of the most careful and conscientious of clinical observers, M. Bernutz, not to mention that it was principally to his researches that we are indebted for our increased knowledge of the true nature of those inflammatory processes, although I am by no means prepared to go so far as he does, and almost absolutely deny the occurrence of cases of perimetric cellulitis at all. The fact seems to be, that while the theory of inflammation of the cellular tissue in the pelvis—a theory almost universally accepted in our own country—was carried too far by the enthusiasm of Nonat in France, M. Bernutz committed an equally grave error by rushing much too blindly in the opposite direction. The truth, as it usually does, appears to lie within the extremes; and though we can easily believe that M. Bernutz has verified his diagnosis more frequently by autopsies in cases of pelvic peritonitis, it must not be imagined, on account of the rarity of such post-mortem verification, that cases of perimetric cellulitis are uncommon.

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[1] Sir J. Y. Simpson, Dr Churchill, Dr West, Nonat, etc.

[2] Grisolle mentions cases of circumscribed pelvic peritonitis simulating phlegmons as early as 1839, but there was really no accurate information until the work of Bernutz appeared.

On the contrary, it may be pointed out, that the very fact of the lesion being so frequently extra-peritoneal is the reason why we so seldom have an opportunity of completing the diagnosis in the way mentioned. No one can for a moment doubt that pelvic peritonitis is a much more serious and grave lesion, and one much more likely to produce a fatal issue, than inflammation of the cellular tissue of the pelvis, and of that alone; and while, on this account, we have seldom any opportunity of confirming the presence of a perimetric cellulitis by post-mortem inspection, such a case as that reported by M. Simon[1] goes far to prove, not only the possibility of its existence, but even the probability of its frequent occurrence.

From this short resumé of the present state of our knowledge of those essentially distinct pelvic inflammations, I now turn to one or two cases which will partially serve to illustrate the symptoms to which they give rise, the treatment we ought to adopt, and the post-mortem appearances in such as terminate fatally.

Margaret H., æt. 39, married, and has had four children, was admitted into Ward XII.,[2] Royal Infirmary, on 6th March 1869, on account of hæmorrhage after an abortion, which had occurred four weeks previously. For purposes of diagnosis the os and cervix were dilated by sponge-tents, and after examination the ergot of rye was given in drachm doses of the liquid extract, thrice daily. The bleeding ceased, and she was about to leave the hospital on 25th March, when she was seized with a rigor after lying in her bed with the adjacent window open. The pulse and temperature rose very considerably. She perspired profusely after the shivering, and became delirious. She also complained of pain above the pubes. The next day, on vaginal examination, there was fulness and tenderness in the retro-uterine cul-de-sac, but nothing more. Ordered opium internally, and poulticing. On the 29th the pain was not quite so severe, and was relieved by her drawing up her knees, and thus removing the tension of the abdominal walls from a slightly tympanitic abdomen. Immediately above the pubes a somewhat hard and very tender swelling was felt, but its limits could not be made out on account of the pain. Vaginal examination proved the existence of a swelling in the posterior cul-de-sac pressing the uterus forward against the pubes, and, by combined internal and external palpation, it was found to be part of the same swelling felt above the pubes. The pulse was 120 per minute. She was perspiring very much, but complained less of the pain.

On the 5th April the swelling seemed softer, and apparently indented slightly on pressure. An exploring needle was introduced, and as pus escaped through it, a tenotomy knife was at once employed to make an incision in the most prominent part of the swelling. The cut was immediately behind the cervix, was nearly an inch in length, and through it there escaped very freely about a pint and a half of very fœtid pus. On introducing my finger through the opening, I felt what I considered to be the peritoneal recto-vaginal fold below. The swelling above the pubes still to be felt, but neither so large nor so sensitive.

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[1] Bulletins de la Société Anatomique de Paris, xxiv$^{e}$ année, p. 234.

[2] Under the charge of Sir J. Y. Simpson.

I need not enter into all the minutiæ of her case given in the Ward Case-book; suffice it to say, that by the 9th April she had decidedly improved. There was little pain, and she could keep the legs stretched out in bed. The opening internally was closing, but the uterus was fixed and slightly anteflexed, and there was fulness and tenderness in the right lateral and posterior regions. By the 16th of the month there was no suprapubic pain remaining, but the uterus, though more mobile, could not be pressed without causing a shooting pain through the abdomen. The improvement continued until the 25th, when she had an attack of an epileptiform character, and it turned out she was liable to similar fits. After them she feels very drowsy, has a vacant, expressionless stare, and does not answer questions put to her correctly. However, after this attack she steadily gained ground, and was dismissed on 1st May, declaring herself to be quite well, though the uterus was still pretty immobile and anteflexed, but somewhat to the left. The wound was quite healed up, and there was no discharge.

This was a highly characteristic case of adhesive pelvic peritonitis following abortion, and ending in abscess. The rapid onset of acute pain, the high degree of inflammatory fever, and the great prostration of the patient from the beginning of the attack, seemed to point to more serious mischief than simple inflammation of the pelvic cellular tissue. Still, many of the symptoms we are accustomed to find in other cases of pelvic peritonitis were wanting in this patient. As in all peritoneal affections, we often have from the commencement of the attack distressing nausea and vomiting, in consequence of which the pulse soon becomes small and quick. Dysuria, occasionally amounting to strangury, and sometimes even leading to complete retention, is often most violent, and I have seen one or two cases in which the attacks of spasmodic pain, apparently at the internal orifice of the urethra, have been so acute as to deprive the unfortunate sufferers of all power of self-control, and to lead them to utter shriek after shriek in their agony. Tenesmus, too, is another of the most distressing of the painful symptoms present, and great pain in defæcation is almost unavoidable, from the ordinary situation of the swelling and effusion. It need scarcely be added, that all appetite is usually lost; that frequently great thirst annoys the patient, and that she lies prostrate in bed, afraid even to stir, and unable to bear the lightest pressure on the wall of the abdomen, and that sleep is but seldom obtained without the use of anodynes. If the inflammation progresses, as it sometimes does, and extends to the abdominal serous membrane, the symptoms of general peritonitis rapidly supervene, and the patients usually succumb. If, on the other hand, the inflammation remains limited, the symptoms become those of chronic pelvic peritonitis, or change into those of intra-peritoneal pelvic abscess.

Still, I do not wish to be misunderstood, nor to lead any one to imagine, because, in a typical case like the one reported, little chance of mistake can arise, that therefore we are able in every instance to make an exact diagnosis. It is only too true, unfortunately, that, in the present state of our gynækological knowledge, we cannot point out any single pathognomonic feature to distinguish pelvic peritonitis from the closely allied pelvic cellulitis; for even though the greater gravity of the symptoms in most cases of pelvic peritonitis cannot be denied, yet it would be highly improper to rely on such a fact for our differential diagnosis. I have seen as severe pain and other symptoms of high inflammatory action in cases of pelvic cellulitis as I have in cases in which I had every reason to suppose the serous membrane alone affected; while, on the other hand, I have been frequently convinced of the existence of sero-adhesive pelvic peritonitis, without the patient presenting such symptoms as would lead us to suspect inflammation in any part of the peritoneum. No doubt, the two affections may often be present coincidently; and, in fact, I have little hesitation in saying, that I believe cases of pelvic cellulitis are very frequently accompanied by some slight peritonitis. Here is a case in point:—

Mrs C., æt. 40, from Liverpool, residing in Lauriston Place, came under my care towards the end of March 1869. Her previous medical history was not very good. She had been married fourteen years, and a year after marriage had a child, but did not make a good recovery, and has never felt very well since. She had been treated by various medical men; had taken a good deal of medicine, especially aperient, and had occasionally improved. She came down to Edinburgh, and was found to have retroflexion of the uterus, for which a loop pessary was introduced. She went about wearing this, although it gave her great pain; and it was only at the end of eight days after its introduction that I was again called to see her. Her symptoms were not by any means marked, the pulse not being above 100, but she could not go about on account of pain referred to the sacrum. On vaginal examination, a swelling between the vagina and rectum was discovered, painful to pressure, but not yielding any sense of fluctuation to combined rectal and vaginal palpation. The uterus was high in the pelvis, not immobile, however, at this time. The cervix was pressed close to the pubes. A few days after my first visit, I found her complaining of supra-pubic pain; and, on examining this region, discovered a tender swelling, which projected slightly above the pubes. Although the swelling behind the vagina was more prominent and nearer the anus, I managed carefully to pass a sound, and proved that this tumour was the fundus uteri. On the eighth day after the first examination, combined rectal and vaginal palpation showed the existence of fluid in the swelling, and I opened it freely with an ordinary bistoury through the posterior wall of the vagina. A good deal of pus escaped, and the tumour above the pubes at once disappeared. On passing my finger into the abscess, I found that it reached, in the cavity formed in the cellular tissue, nearly to the anus. The pus continued to flow for about a week, and a month afterwards she left Edinburgh, having made an excellent recovery, and declaring herself to be freer from pain and uneasiness than she had been for years. Her state then, as given in my note-book, was as follows:—"No pain from deep pressure in the supra-pubic or iliac regions. Internally, uterus pretty immobile and acutely retroflexed, but somewhat laterally towards the left sacro-iliac synchondrosis. Pain on pressing the cervix upwards, and there is still some slight ulceration of the os uteri. She eats and sleeps well, and, though weak, is able to walk about without pain. The incision is quite healed, but there is some fulness remaining in the recto-vaginal space."

The situation of the cellulitis in this patient between the rectum and vagina is by no means the most common, West, in his work on the Diseases of Women, placing only 14 out of 51 cases in this position. The most common seat of cellulitis is said to be within the folds of the broad ligaments. But, even setting aside the difficulty of accurately ascertaining the exact site of those inflammatory products without a post-mortem examination, it must be remembered that Nonat, West, and M'Clintock, who give statistics or offer opinions on this subject, do not sufficiently discriminate between the intra and extra peritoneal forms of pelvic inflammation, thus rendering any assertions on the point almost valueless. For similar reasons, it must be equally apparent that we have at the present no reliable information on the subject of the comparative frequency of suppuration in the two distinct forms of inflammation in the serous membrane or cellular tissue of the pelvis. Thomas, indeed, with a boldness worthy of a better cause, roundly asserts that the marked tendency to suppuration in cellulitis, is an important point in the differential diagnosis of that affection from peritonitis; but, though other authors, as Duncan, maintain that suppuration is the most common termination in cellulitis, yet no one, so far as I know, except Thomas, has ventured to draw any important inference from what is at the best a mere supposition on his part.

There can be no doubt, however, that when pus has formed in the cellular tissue, it much more frequently finds an exit for itself externally than internally. The statistics of M'Clintock—though liable to correction on account of the fact just stated—

seem to show that it is through one or other of the iliac regions that the puerperal cellular abscess most commonly discharges itself. But when M'Clintock goes on to express his concurrence with Dr Bennett in the statement, that non-puerperal pelvic cellular abscesses are most frequently discharged through the rectum or vagina, I cannot help affirming that he offers no adequate proof for his belief. The passage, however, in M'Clintock's essay on the subject is so confused, that I am quite unable to make out whether he is really speaking of cellular abscesses only, or whether he does not include intra-peritoneal collections as well. "I have an opinion," says Duncan,[1] "that abscesses in the cellular tissue (parametric abscesses) open more frequently externally than intra-peritoneal abscesses (perimetric abscesses) do;" and Aran seems to be much more convinced of the truth of the assertion than Duncan is. At least he states very decidedly his opinion, that intra-peritoneal abscesses almost never find an exit through the abdominal walls; and on anatomical grounds alone, we may be fully as confident of this as Aran. The fact seems to be, as Duncan has already pointed out, that the intra-peritoneal abscesses perforate the most perforable viscus; and if we reflect for a moment on the most common site of the purulent collection in those cases, it becomes evident that the rectum and sigmoid flexure are the parts of the intestines into which the matter is most likely to find its way. Very seldom, indeed, does the perforation occur through the bladder, and even more rarely through the uterus or vagina.

That suppuration is more common in the puerperal[2] than in any other variety of pelvic inflammation—whether in the serous membrane or cellular tissue—is a fact disputed by no one; and I think it also probable, that not only is suppuration much the most common termination in such cases, but also that the matter forms much more rapidly. In one patient, in whom the peritonitis had been caused by exposure to a cold wind after an abortion, the pus was completely matured, and the abscess ready for opening on the seventh day after the existence of fulness in the retro-uterine cul-de-sac had been made out. There are one or two other events, too, which have been noticed by observers as more frequent in the puerperal form of the disease than in any other. M'Clintock, for example, calls our attention to the very insidious manner in which the attacks sometimes come on; and in reference to this makes the admirable observation, that in all cases of bad recovery from childbed, we should carefully examine the iliac regions day by day, as he has thereby frequently been enabled to detect latent inflammation when no other symptom indicated the occurrence of morbid action. In making this remark, he is, of course, speaking of cellulitis, the only variety of pelvic inflammatory tumour he attaches any importance to; but if we add, that in many such suspicious cases, it is absolutely necessary to have recourse to internal examination, the observation is equally valuable with reference to pelvic peritonitis. Bernutz, too, notices another danger to which the puerperal patient is much more liable than the non-puerperal, when pelvic peritonitis has been developed. "The extension of the inflammation by simple contiguity from the pelvic to the abdominal peritoneum," he observes, "is especially liable to occur in cases of puerperal pelvi-peritonitis at about the end of the second week when the patient has not taken sufficient care of herself. This happens much more frequently in hospital than in private practice." The following notes of the post-mortem examination[1] of a patient who died in the Royal Maternity Hospital, tend to show that this observation of Bernutz is unfortunately only too often verified:—

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[1] On Perimetritis and Parametritis, p. 160.

[2] Under this head, of course, are to be included the cases coming on after abortion.

The patient, aged 29, miscarried at the seventh month, and was delivered of a dead child, after a labour lasting nearly forty hours. She died ten days afterwards, her symptoms all along having been very obscure, and the termination sudden. At the autopsy the cause of death was found to be general peritonitis, but the important point, as bearing on our subject, was the fact that the posterior part of the pelvis was hollowed out into a large cavity, containing about a couple of pints of sero-purulent fluid, which had principally gravitated into the lower part of the retro-uterine cul-de-sac. This cavity was formed by the still enlarged uterus, ovaries, and broad ligaments in front and to the sides, by adhesions of the fundus uteri to the intestines above, and by the rectum and peritoneum behind. The adhesions were recent and easily removed, and the rectum and posterior surface of the uterus were covered by recent lymph. The internal surface of the uterus presented the ordinary appearance of a puerperal uterus. There was no endometritis and no inflammation in the mucous membrane of the right or left Fallopian tubes. The posterior surfaces of both broad ligaments were covered by lymph. The right ovary was much enlarged, being two inches in length. On its anterior surface, and close to the Fallopian tube, a small rounded orifice about the size of a fourpenny-piece was found. Its edges were smooth. On the lower border, another, but larger, rounded orifice existed. It was about the size of a shilling, and had ragged and everted edges. These two orifices communicated with each other, and the ovary between was excavated out into an irregular cavity, which, however, contained no pus.

It cannot be denied that cases similar to this have frequently occurred, and it is almost equally certain, that in very many of those cases, and up to a very recent time, so much confusion existed in the minds of observers as to the nature of the malady, that the patients attacked with pelvic peritonitis after delivery were supposed to be labouring either under metritis, or, in bad forms, under one or other of the manifold forms of that Protean disease—puerperal fever. No doubt, in nearly every case of what is called puerperal fever, we have pelvic peritonitis, but this is only one of the many local manifestations of a constitutional state. On the other hand, in such a case as the one just reported, we can usually refer the inflammation to some specific cause; and the existence of septicæmia, or the presence of a contagious element, do not enter into our speculations as to its origin. Thus, we see at once that it was the bursting of a small intra-ovarian abscess, during a lingering labour, that gave rise to the peritonitis, which, by extending to the abdominal serous membrane, caused the death of this patient.

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[1] The case has already been brought under the notice of the Obstetrical Society by Dr C. Bell, and will be found at greater length in the Edinburgh Medical Journal for August last.

There can be doubt that all the forms of intra-peritoneal abscess, from whatever cause arising, are incomparably more dangerous than those resulting from pelvic cellulitis; and even though the matter be favourably situated for evacuation, we ought in such cases to have a constant dread of general peritonitis, and its almost invariably fatal issue, and we must use every means in our power to avert such an unfortunate result. Those abscesses, it is true, are usually encysted within walls, formed either by fibrinous deposit, or by the matting together of two or more of the pelvic or abdominal viscera; but even though such walls may be very useful in preventing the lighting up of a general inflammation, yet the constant production of matter from the newly-formed pyogenic membranes is very apt to lead to the exhaustion of the patient, especially if the pus has found an outlet, whether externally or internally. We must always bear in mind, too, that the walls of abscesses formed in this way are, at any rate at first, not very strong, and that instances have occurred where rough handling, either through the abdominal parietes or per vaginam, has ruptured the fragile membranes, and caused the exact result we ought so carefully to avoid. Another danger which often puts the lives of such patients in jeopardy, in whom the abscess opens into the bowels, is the long-continued exhaustion which sometimes results from the chronic diarrhœa or dysentery thus produced; nay, more, I have a most vivid recollection of the unutterable agony undergone by one lady from the frightful attacks of tenesmus, caused by the passage of such acrid matter over the sphincter of the anus,—an agony which, yielding to no remedy but chloroform, was in itself sufficient to have killed the unfortunate sufferer.

In every attempt which has as yet been made to classify cases of pelvic peritonitis according to their causation, the puerperal variety, including those cases coming on after abortion, has been universally allowed to be the most frequent. The truth is, however, that though more than half the cases would be found to come under this head, yet we can seldom be quite sure, without the help of an autopsy, of the primary disease which has given rise to the pelvic peritonitis, and as to whether that primary disease is of uterine, tubal, or ovarian origin. Some authors, certainly, do not seem to have much doubt on this subject. Thus, Aran, in his "Leçons Cliniques sur les Maladies de l'Utérus," most distinctly gives it as his opinion that the primary affection is almost invariably ovarian or tubal. At p. 663[1] of that work, he says, "The true element, that playing the principal part in what we call periuterine inflammation, is the alteration of the uterine appendages, the ovary, or tube." Again, at p. 673, "Perhaps inflammation of the uterus, especially endometritis, plays some part in the invasion of periuterine organs by inflammation, independently of any alteration in the ovary or tube. This I can say, that I have as yet met with no fact of the kind,[2] and am not completely convinced by any known facts which might seem to support an opposite opinion. It has not been shown to my satisfaction that the ovaries and tubes have been examined with sufficient care. Too frequently the examination has been made months, nay, years, after the attack, when any traces of inflammation in these organs may have disappeared long before." Again, at p. 679, we find him making no less positive statements, as to the manner in which the inflammatory swellings are formed. "When the appendages of the uterus are affected only on one side," he says, "it is from those appendages, as from a centre, that the inflammatory changes extend, and the situation of the diseased ovary or tube fixes definitely the site of the tumour in front, behind, or on either side of the uterus. But if, as in a number of cases, the appendages are attacked on both sides at once, the pathological changes soon meet in the mesial line, most frequently behind, but sometimes in front of the uterus, surrounding that organ in a kind of ring, which is more or less complete." From these quotations, and from many other passages of the work, in which the same idea is reiterated, it will be seen that M. Aran makes the uterus play a very subordinate part in the development of pelvic peritonitis; and Dr West, in his admirable treatise on Female Diseases, takes up the same notion, and expresses the same opinion in almost identical terms. But we can scarcely hesitate to believe, that in many of the cases the uterus is the real starting-point of the inflammation. Indeed, it has lately been stated,[3] that the extension of endometritis along the tubes is the most common cause both of the puerperal and non-puerperal varieties of the malady we are discussing. However true this may be of the puerperal and gonorrhœal forms of pelvic peritonitis, I am not as yet inclined to believe in the frequent causation of other varieties in this way. I have seen limited inflammation in the serous membrane so rapidly developed after the introduction of a tangle-tent, the employment of a cutting or burning instrument to the uterus, or even after the use of the sound, that no inflammation in the mucous lining[1] of the interior of the womb could have had time to spread through the tubes to the peritoneum.

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[1] See also pp. 667 and 718.

[2] Dr M. Duncan has, however, managed to convict M. Aran of inconsistency in his statements. *Vide* Duncan on Perimetritis and Parametritis, p. 41.

[3] Duncan, *op. cit.*

That the determining causes of the puerperal variety of pelvic peritonitis are much the same as those that might give rise to metritis or endometritis, may be adduced as an argument in favour of the opinion that the affection of the serous membrane is merely an extension by contiguity of the inflammation in the womb. Out of eighteen cases given by M. Bernutz, we find him stating that four arose from difficult parturition; three from cold; nine from too early rising after confinement; one from fatigue; and one from venereal excess; and M'Clintock observes the same fact as to the frequency of its origin from too early rising, as he mentions that Dr Heyerdahl of Christiania had informed him that, so frequently were patients attacked with pelvic cellulitis after leaving the Maternity Hospital in that city, that the managers had felt themselves compelled to lengthen the duration of residence in hospital, to prevent, if possible, such accidents in the future.

Bernutz dwells more particularly on the fact of the extension of the inflammation from the mucous membrane, when he enters on the consideration of what he calls blennorrhagic pelvic peritonitis, and he concludes, from his observations in the Lourcine Hospital,[2] that it is a very common variety indeed, and that pelvic peritonitis from this cause is not only the analogue of orchitis in the male, but also that it occurs in about the same proportion of cases as does that extension of gonorrhœal inflammation in men. Nay, so far does Bernutz drive his analogy, that he even occasionally gives to the whole group of symptoms of a case of pelvic peritonitis the name of *feminine orchitis*.[3]

Very few physicians in this country have had the same opportunities of studying the gonorrhœal variety as Bernutz has had at the Lourcine Hospital; and, in consequence, we find very few cases of blennorrhagic pelvic peritonitis in the writings of our gynækologists. So far as I can remember, I have only seen a single case of this interesting variety, but that a most formidable one, though it did not end fatally. In this woman I had the opportunity of treating the case from the commencement of the vaginitis to its termination in oophoritis, and inflammation of the serous membrane round the ovaries, and of tracing the gradual progress of the disease through its successive stages. It did not differ materially from any of the cases of a similar nature given in the work of Bernutz, except that the pelvic peritonitis was not developed, or, at least, was not evident until fully three months from the time when the gonorrhœa had been contracted, whereas the observation of Bernutz has not as yet presented him with any case in which the lighting up of the serous inflammation was delayed more than two months from the first appearance of the purulent discharge. In relation to this point, however, Dr Duncan judiciously observes that the dangerous period after delivery, after the commencement of a gonorrhœa, or after an uterine operation, lasts much longer than is generally supposed,—an observation, the full importance of which can only be appreciated by those who remember its practical bearings on prognosis and treatment.

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[1] I do not here enter into the question whether metritis itself can be propagated by contiguity to the peritoneum. Metritis of the non-pregnant or non-puerperal uterus must, indeed, be rare, when Klob declares that he has seen no case in which he could positively say a "genuine metritis" existed. (Klob, Pathologische Anatomie der Weiblichen Sexualorgane, s. 209.)

[2] The Lourcine—the Female Syphilitic Hospital in Paris—offers, of course, exceptional statistics on this subject. During several months of the year, Bernutz tells us that, owing to want of space, they were obliged to admit as in-patients only those who presented the symptoms of periuterine phlegmons.

[3] Properly speaking, it is the oophoritis which is the analogue of orchitis, but it is impossible in such a case to separate the inflammation of the tissue of the organ from that of its serous membrane.

Amongst the different forms of pelvic peritonitis, one of the most common, and one of the most important, is the menstrual, under which head Bernutz includes 20 out of his 99 cases. In the majority of those patients the inflammation arises from the sudden suppression of the menstrual flux, whether that be caused by cold, by some mental emotion, or by any injury during the period. In almost all such cases which terminate favourably, it will be nearly impossible for us to detect the part primarily affected, although, as we believe, there must necessarily be some starting-point or other for the inflammatory action. In the following case I suspected that the right ovary was the *origo mali*:—

Mrs G., æt. 30, from Forfarshire, was suddenly attacked on the second day after the commencement of the menstrual period with most violent pain in the right side of the pelvis, and when I saw her a few hours afterwards was labouring under symptoms referable to pelvic peritoneal inflammation.[1] There was nothing to be made out of external examination, and on account of the menstrual flow I made at that time no internal exploration; but the next day I discovered by palpation a small and very tender swelling above the right supra-pubic region. Internal examination—the period ceasing the same day—discovered the uterus nearly immobile in the pelvis, its fundus anteflexed, and at the same time drawn over to the right, and the cervix high up and looking towards the left. There was great tenderness in the right lateral region and posterior cul-de-sac, and a feeling of fulness, but the left side was quite free. Any pressure on the cervix at once caused a darting pain in the seat of the swelling to be felt through the abdominal walls. The bladder and rectum unaffected.

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[1] I had seen this lady about a week before, when she had complained of some pain over the cæcum, but this had yielded to very mild remedies, and she had been going about and feeling perfectly well until the sudden attack during the menstrual period. I was called to attend her in the absence of Sir James Simpson on the Continent.

Pain very excruciating, and only relieved by repeated subcutaneous injections of morphia. The swelling increased rapidly in size for the next few days, until it reached about three fingers' breadth above the pubes, at the same time extending outwards towards the anterior superior spine of the ilium. The pain continued most violent, and all the ordinary symptoms of peritonitis were present. The pulse was small and rapid, keeping about 120 per minute, and vomiting, which began on the third day, and lasted for about twenty-four hours, seemed to yield finally only to small doses of dilute hydrocyanic acid. About the fifth day, and coincidently with the subsidence of the vomiting, the pain became more endurable, the pulse sank to about 100, and the tension of the abdominal walls and tympanitis diminished. The tumour, though still very tender to the touch, could then be felt from without to be distinctly circumscribed, and did not extend more than two inches beyond the symphysis. Percussion over it elicited a slightly tympanitic sound, except just above the pubes, where it was also most tender to pressure. After the sixth or seventh day from the commencement of the attack the lady did very well, but the pain remained very obstinate, and was always worse down the front and inner side of the thigh. Still it was not relieved in any degree by keeping the thigh flexed. The swelling began slowly to disappear, and on internal examination, about the twelfth day of her illness, the uterus was found to have regained some of its mobility, though pressure on its cervix or up against the fundus still caused the violent shooting pains already described. The ante-lateral flexion remained the same.

On the sixteenth day from the beginning of the attack, I found that the swelling was scarcely to be felt through the abdominal wall, except by deep pressure into the pelvis, which still caused the patient very great pain. The uterus in much the same state, but its mobility undoubtedly greater. She was at that time eating and sleeping fairly, but had begun to complain of pain in micturition. She continued slowly to improve, and there were no symptoms of suppuration present, when, rather to my surprise, on the twenty-fourth day pus appeared in the water in very large quantity; and a day or two later nausea and vomiting began again, and proved most obstinate, resisting all the various methods of treatment I will presently detail. She had ultimately to cease taking any food or liquid by the mouth, being nourished solely by nutritive enemata. Her pulse, however, during this time never rose above 100, and she managed to get a fair amount of sleep, though chiefly by means of the subcutaneous injections. The pus continued to appear in the urine for about a fortnight, gradually diminishing, however, in quantity—the acts of vomiting, too, becoming much less frequent; and, without entering into further details, she was convalescent, and able to be out of bed about the middle of the seventh week. The uterus had then regained much of its mobility, but was still anteflexed. A month or two afterwards she went to travel on the

Continent, and when I saw her on her return she told me she had not suffered in any way from fatigue or exertion. I ought to have mentioned, that this lady had only had one child some ten years before, when she was delivered with great difficulty. The labour occurred in Italy, but she could give me no particulars of it, except that it had lasted three days. She also stated that she had suffered from similar attacks to the one I have just described, but not so severe.

The swelling—for, strictly speaking, it could not be called a tumour—which formed so rapidly in this patient was obviously formed by certain portions of bowel agglutinated together by lymph, and at the lower part probably adherent to the right ovary, and possibly also to the fundus uteri, which was drawn over to that side. That the swelling was mainly formed by intestine is evident from the fact that percussion over the upper part of it elicited a modified tympanitic sound, which changed into a dull note nearer the pubes. Indeed, the hard and resistent tumour we find in similar cases of pelvic peritonitis is mainly due to the matting together of the pelvic viscera by the deposited lymph, which, becoming stronger and firmer, produces very dense fibrous adhesions; and also to the accumulation of sero-purulent or purulent fluid in the loculaments formed by such bands. It is owing to these facts that chronic pelvic peritonitis has not unfrequently been confounded with ovarian and fibroid tumours.

The reasons which led me to suppose that the right ovary was the original cause of the inflammation in the serous membrane in this patient are chiefly these:—

*1st*, The occurrence of the attack at a time in which the ovaries are especially the seat of congestion.

*2d*, The position of the swelling.

*3d*, On internal examination, after the subsidence of all inflammatory action, I discovered an enlargement to the side of the uterus, which was in the position of the right ovary, and still sensitive to pressure.

*4th*, The statement of the patient, that she had had four similar attacks at periodical intervals of two years, and that on each of these occasions she had suffered from the same violent pain chiefly in the right side and leg.

It may, however, at once be stated, that we never can be positively certain of the real cause of the attack without the aid of an autopsy, and even not always then, as the traces of the original disease may either have completely disappeared, or have become merged in those of the secondary affection.

Amongst the many forms of pelvic peritonitis, the traumatic variety does not take a very prominent place in the systematic works on the subject, although, in my opinion, its importance is considerably underrated. I am compelled to express my conviction also, that the ᵃmmation of the serous membrane in many instances of this variety is due either to some imprudence on the part of the patient, or to some want of caution on that of the physician, although cases of course occur in which neither would seem deserving of blame. Thus, I distinctly remember, that in one patient the pelvic peritonitis was apparently produced by the passing of an uterine sound to measure the cavity. The peritonitis in this case—a weak exhausted woman of about forty—spread rapidly to the abdominal serous membrane, and proved fatal in some eight or ten days. At the autopsy no sufficient cause for the development of the peritonitis could be found; there was no endometritis or inflammation in the Fallopian tubes.

Among other determining causes of the traumatic variety, I may mention the use of tangle-tents to dilate the cervix, several examples of pelvic peritonitis arising in this way having come under my notice. Cutting operations on the uterus, the excision of polypi, the application of solid nitrate of silver to the interior of the womb, or of caustic potash to the cervix, and the wearing an intrauterine pessary, have all proved the exciting causes of such cases; and among others which have been mentioned, and which we can readily conceive may have produced the inflammation, we find recorded venereal excesses, injections of fluid into the uterine cavity, and chancres on the cervix.

I cannot conclude these observations without making some allusion to the important subjects of diagnosis and treatment, and it is with some shame that I confess that much, very much, remains to be done before we will be able to publish any incontrovertible opinion on either point. The greater number of gynækologists, in attempting to establish a differential diagnosis between those affections, of the serous membrane, and of the cellular tissue in the pelvis, either simply repeat the leading signs and symptoms they imagine to be peculiar to each, or even fail more lamentably by ambitious, if praiseworthy, attempts to establish certain points in the diagnosis, which they consider to be infallible. In a work[1] only very recently published, we may see an instructive example of the complete failure of such an experiment. Thomas has drawn up a long series of differential points, of which it may be said that in some cases his statements would answer for both complaints, while in many others they might be reversed without impropriety. As no one, then, can point out any one or two features so salient as to guide us by their existence to the immediate separation of the one form of inflammation from the other, it would perhaps be as well to abstain from doing so at all, lest in attempting to avoid the Scylla of over-positiveness we fall into the Charybdis of obscurity. I shall endeavour, however, in as brief a space as possible, to indicate the direction at least in which our attention must be turned when we are trying to separate the two affections.

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[1] Gallard Thomas, A Practical Treatise on the Diseases of Women, p. 375. (Phil. 1868.)

In a former part of this paper, I expressed a belief that we would be committing a grave error if we were to rely on the greater severity of the symptoms in cases of pelvic peritonitis than of cellulitis; and, in repeating this statement, I am necessarily forced further to point out that it is to the physical signs we must chiefly trust to guide us correctly; and certainly I can confidently say, that there are few more difficult points in physical diagnosis than this, the difficulty being in many cases greatly increased by the fact that, unless under chloroform, the combined internal and external manipulation necessary to satisfy us is usually at first too painful for the patient.

In by far the greater majority of cases, then, of pelvic cellulitis, we find, on internal examination, that the induration or swelling is on one or other side of the uterus, and that it is only very rarely that the whole roof of the pelvis seems blocked up by the hard stony mass so common in cases of pelvic peritonitis. The tumour in most cases of cellulitis is found principally in one or other of the iliac fossæ, and apparently closely connected to the iliac bones, or, if it has not advanced so far, to the pubes; while the tumour, if felt at all through the abdominal walls in peritonitis, is usually more in the mesial line, or, if situated laterally, does not by any means seem so intimately blended with the bones. An important point in the diagnosis of pelvic cellulitis, as well as of pelvic peritonitis, is the immobility or fixation of the uterus—an immobility which, in many cases of peritonitis when it is fixed all round, may be said to be absolute, while in cellulitis it rarely if ever happens that the fixation is so complete. As the immobility arises to a great extent from the presence of adhesions uniting the womb to other organs or viscera, and not alone from the presence of what we may call a tumour in the pelvis, we can easily conceive why it should be more marked in inflammation of the serous than of the cellular tissue.

In cellulitis, the uterus is usually pushed to the opposite side of the pelvis from that occupied by the phlegmon, whereas, in some cases of peritonitis, the body and fundus of the womb are so completely lost or enveloped amidst the exudation[1] that it is only by the use of the sound, when such a use is justifiable, that we can determine its exact position in the pelvis. There can be no doubt, too, that flexions of the uterus of one kind or another—ante, retro, or lateral flexions, according to the position of the swelling—which are common in peritonitis, occur much more rarely in cellulitis. When the swelling has risen into the abdomen, we may be partially assisted in our diagnosis, not only by observing the position of the tumour, or by having followed its mode of origin and progress, but also by calling to our assistance such aids as percussion and auscultation. Portions of the intestine so frequently enter into the composition of the intra-peritoneal tumours, that we can often obtain from them a more or less modified tympanitic note, or hear the movements of gas or fluids through the mass. I know of only one form of cellulitis that could be confounded with an intra-peritoneal tumour under those circumstances, and that is the extension of the phlegmon to the abdominal cellular tissue, the *external peritonitis* of some authors. There is one point which every gynækologist can corroborate, namely, the retraction of the thigh in cases in which the cellular tissue round the psoas and iliacus muscles is involved, and the relief of pain which this flexion produces. I have frequently noticed, in cases of pelvic peritonitis, that the flexed position of the thigh either did not ameliorate the pain, or did so only when both knees were raised, by removing the tension of the abdominal muscles.

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[1] See Aran, *op. cit.*, p. 680.

To go any further to attempt the separation, for instance, of the different varieties of pelvic serous, and cellular inflammation from one another, or those again from other diseases for which they might be mistaken, wóuld lead me far beyond the scope of my present observations. I have said enough, I hope, to show that, while in a few cases the most experienced practitioner may without shame confess himself at fault, yet, in the majority, the whole of the features of the case, the *tout ensemble* of signs and symptoms, present to our minds so many points of sufficient distinctness as to enable us to form a correct diagnosis.

If most of our gynækological authors hesitate, however, to lay down any definite rules to guide us in our diagnosis, many of them are, on the other hand, rather too dogmatic on the subject of treatment. In this country, at least, it is not likely that many of us will be inclined to follow the rules which Nonat considers so infallible, and adopt his *coup-sur-coup* plan of bleeding, or to assent nowadays to Aran's doctrine of the benefit to be derived from mercurialization. There may still be among us practitioners who think that we have rushed too blindly from one extreme into the other, and who remind us of the great alleviation of pain that bloodletting brought with it; but certainly there must be few, very few, left who can unhesitatingly approve of the decidedly antiphlogistic treatment of one section at least of the French school of medicine. As I must emphatically reject, then, any such violent plans of treatment as have been just mentioned, it appears necessary to say a few words as to the method which it seems most advisable to pursue—a method which is based chiefly on the doctrines taught in our own University.

Before doing so, however, I wish to remark, that many of the cases of pelvic inflammation might possibly be prevented by a proper regard to prophylactic measures, and it is in this respect that the observation already quoted from Dr Duncan's work on Perimetritis and Parametritis seems so valuable. The experience of the Directors of the Christiania Lying-in Hospital, as reported by M'Clintock, proves to us, for instance, how necessary it is both to attend to puerperal patients, by preventing them returning to work too soon, and not to neglect even the slightest anomalous symptom occurring in women in this state. Many cases, again, of the gonorrhœal variety might have been prevented, if the practitioner had properly interpreted the indications which the extension of the specific inflammation to the mucous membrane of the womb presented. I have already dwelt on the absolute necessity there exists for great care on the part of both patient and physician when the cervix is to be dilated, when any cutting or burning operation is required, or when we intend to apply any curative remedy to the interior of the uterus, or to inject its cavity. Suffice it to say, that the surprising rapidity with which pelvic inflammation so frequently supervenes from even the most trivial traumatic influence, will soon teach the young and inexperienced practitioner the necessity for caution.

In the acute stage, then, of either of the forms of pelvic inflammation we have spoken of, it may be necessary to be so far antiphlogistic as to remove blood by leeching. Of course the amount to be taken from your patient is to be regulated by her general strength, and according to the form of inflammation and variety of the disease she is suffering from. A phlegmon will require the removal of blood to a less degree than a circumscribed peritonitis. A puerperal patient, again, will probably be seldom able to bear much additional loss of blood: the sufferer from the gonorrhœal variety is usually too much weakened by the long-continued discharge which generally precedes the outbreak of the secondary affection: and the same will, in all likelihood, be true of those in whom the local peritonitis or cellulitis is only the complication of an organic disease. In the menstrual variety, on the other hand, when the flow has been suddenly checked, and in most of the traumatic forms, leeching, even to the withdrawal of a considerable amount of blood, will not only prove useful, but may even be imperatively required. The rules and method of leeching have been dwelt on at so great length by many of the French authors, that the variety of directions given, and the number of precautions they say are necessary, might almost drive an ordinary practitioner to distraction, if he were to attempt to pay any attention to them. For my own part, I have never yet seen a case in which I thought it essential to take the blood from the cervix direct. I have, in every case I employed leeches, attained all that I desired by allowing them to fasten round the anus; and the communication between the hæmorrhoidal veins and those of the uterus and its appendages, is so free that we may almost consider this to be direct leeching. My great objection to the introduction of a speculum, of such a size and shape as to be suitable for leeching, into a hot and irritable vagina, is that it must press injuriously in nearly every case against an inflammatory mass, an immobile uterus, or a displaced and tender ovary.

But if the local detraction of blood is required more in pelvic peritonitis than in the simpler form of pelvic inflammation, the use of opiates may be said to be even still more imperatively demanded in the former. The continued and severe pain, the obstinate vomiting, and the early exhaustion of the patient in consequence of the retching and straining, force us to keep our patients as much as possible under the influence of narcotics, and the most powerful of those remedies are usually necessitated. Opium and morphia suppositories of the strength of a fourth of a grain and upwards, repeated at frequent intervals, and the subcutaneous injection of strong solutions of morphia,[1] are generally the best forms for the administration of that valuable narcotic, as the nausea and sickness often prevent us giving anything but ice by the mouth. Other anodynes, such as cannabis Indica, conium, hyoscyamus, etc., may be used, and sometimes are required, but I must confess to having little confidence in their pain-destroying properties in such acute affections as inflammation of the pelvic peritoneum, although they may perhaps be found useful in the few cases in which you ascertain that there is a distinct idiosyncrasy on the part of your patient against the use of opium.

Besides these general and local means, we must have recourse at the commencement of all such cases to constant poulticing or stuping, and it is generally advisable to add to the poultice some soothing tincture, liniment, or oil. The French, in cases in which there is much tympanitis, recommend the application of ice-bags over the abdomen; but most English physicians declare their prepossession in favour of the older method of using heat and moisture combined, in every case of peritoneal inflammation. From mercury, in any shape or form, I have never seen any benefit accrue; but to those who are wedded to the notion that its action is necessary for the resolution of a serous inflammation, it has been suggested[2] that the best form in which it could be given, would be that of medicated pessary or suppository. In both ways, a slight action on the gums can very soon be obtained. In the first week or ten days of the acute stage, very little good is to be had from acting on the bowels by purgatives, though occasionally you require, by means of some simple enema, thrown up through a long tube,[3] to remove any hard or impacted fæcal masses, which, by accumulating in the sigmoid flexure or rectum, may be pressing injuriously on the tender swelling in the pelvis, and thus aggravating the sufferer's pain.

The vomiting is another of those distressing symptoms which frequently requires our interference, and, I must confess, very often baffles all the ingenuity and skill of the medical attendant, occasionally proving even so exhausting as to destroy the patient. Small doses of dilute hydrocyanic acid, bismuth in powder or solution, oxalate of cerium, charcoal, cherry laurel, or lime water, small doses of opium, blisters to the epigastrium, the spinal ice-bag,[1] and the constant use of ice may all be tried in rapid succession, and each successive remedy prove as futile as the one preceding. Sometimes we are obliged to abstain from giving food or medicine of any kind by the mouth, and to trust to nutritive enemata solely, though in a case of pelvic peritonitis the necessity which should induce us to do so must be dire indeed, as one of the main objects of our treatment ought to be to keep the lower bowel as quiet as possible.

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[1] In the last case I began with a quarter of a grain of the bimeconate of morphia in solution, but had rapidly to increase it first to one half, then to a whole grain, and finally to a grain and a half, twice a day, to keep the intense pain in subjection.

[2] Sir J. Y. Simpson's Lectures on Diseases of Women.—Medical Times, 1859–60.

[3] See Aran, *op. cit.*, p. 737.

When the active symptoms have subsided, and the complaint has passed into the chronic stage, the treatment will vary according to whether we have an abscess formed or not; but to enter even superficially into a discussion of either point would lead me too far. In the two cases to which I have just alluded, and in which I evacuated the pus, the success justified the treatment in my eyes, whatever it might do in those of such an opponent of active interference as Aran. I would only call attention to what others I believe have also stated, which is, that an abscess may exist without our being aware of the fact. I have occasionally been startled by the appearance of pus from the bowel or bladder, when I had not even the faintest suspicion that suppuration was going on in the pelvis. Nonat, it is true, maintains that the occurrence of suppuration is almost invariably due to the neglect of intelligent treatment on the part of the practitioner; but as his intelligent treatment consists in repeated venesections, I am afraid that most of our brethren in this country are likely to remain great sinners in his estimation. Perhaps I may be allowed to point out, however, that we may sometimes prevent the disease becoming chronic by carefully guarding our patient from imprudent acts until at least the first menstrual period has passed, and thus warding off those exacerbations of the original evil, the *redoublements inflammatoires*, which are so trying to the patience both of physician and patient.

In quitting the treatment of those important affections, I cannot help quoting a passage from Bernutz:—"I ought to add," he says, "that I have found conium a most valuable special narcotic to the genital organs. I give it so as just to produce slight derangement, and a kind of hallucination." I have never tried it, but an observation of the kind from so attentive a clinical physician as Bernutz is very valuable.[2]

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[1] In two very severe cases, I imagined that the spinal ice-bag checked the vomiting, when all other remedies had failed, but no opinion can be founded on two cases.

[2] Compelled by the state of my health to winter in the south of England, I have been quite unable to get at many of the books, periodicals, or journals alluded to, and have been obliged to trust to my memory to a much greater extent than desirable. I must therefore crave the indulgence of my readers for any inaccuracy in reference or quotation they may notice.

## ARTICLE V.—*Case of Popliteal Aneurism treated by Compression.* By PATRICK HERON WATSON, M.D., F.R.C.S., Lecturer on Surgery; Surgeon to the Royal Infirmary, and to the Chalmers Hospital for Sick and Hurt.

FRANCIS JOHNSTONE, æt. 30, was sent to my care by Dr Thomas, of Selkirk. He was admitted to the Royal Infirmary on the 23d September 1869, suffering from a large aneurismal tumour, occupying the lower third of the inner side of the thigh. The knee was persistently slightly flexed in walking, which was effected by the aid of a stick—limping fashion.

The tumour when examined presented all the characters of an aneurism. It extended from the lower border of the sartorius muscle as far as the middle of the popliteal space, and at its centre increased the circumference of the thigh by three inches. The whole limb below the tumour was venously engorged and swollen.

The swelling had only existed for six months, but the limb had been the site of painful rheumatic sensations for more than three years, the swelling first manifesting itself at the point where the pain had been most intense.

*Friday, 24th September.*—The pulsation, thrill, and bruit, together with the pain, are all exaggerated; I accordingly shaved the surface, and commenced the compression by means of my own apparatus, applying the weight, for reasons I shall afterwards give, over the superficial femoral artery, just above the sartorius.

*25th.*—The compression has been diligently superintended by my house-surgeon, Mr Bentley, and continued till to-day at visit, *i.e.*, for twenty-four hours. Throughout all this period there had been neither pulsation, thrill, nor bruit discernible, and now the sac is firm and resistent. There is no pulsation in the superficial femoral, but a powerful pulse in the common femoral and profunda. The gum-plaster, which had been applied over the groin, is loosened, by the copious transpiration from the cutaneous surface, and came away whenever the weight was removed. The patient said, during the first six hours the pressure of the weight occasioned him great uneasiness, but that since then there had been no pain of any kind, except a sensation of burning in the tumour and inner side of the calf beneath this point.

The limb was wrapped in cotton and laid comfortably on pillows.

*26th.*—There is slight pulsation recognisable in the most prominent part of the tumour, but there is no pulsation to be recognised in the superficial femoral.

*27th.*—The aneurismal symptoms occupy the whole extent of the tumour.

*28th.*—The superficial femoral is again pervious, and the aneurismal symptoms exactly as they were before the employment of the compression.

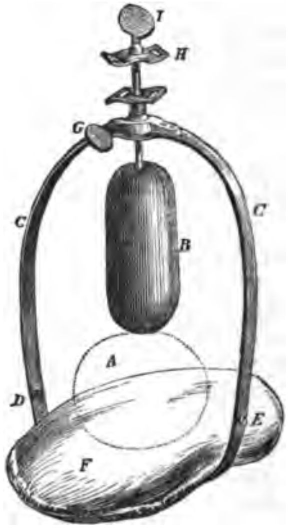
The groin was again shaved, and the whole of Scarpa's triangle and lower part of the belly on the right side having been thickly dusted with fullers'-earth, the weight was now adjusted so as to compress the *common* femoral artery, and, if required, the external iliac. Here it was retained under the surveillance of my house-surgeon and dressers for forty-eight hours without intermission, and without the slightest recognisable pulsation, thrill, or bruit. The patient had experienced no pain at the site of compression, but the same burning sensation in the sac and down the limb was experienced after the lapse of about six hours.

There has been no return of any aneurismal symptoms since the period when the compression was reapplied; the patient has, however, been retained in hospital to admit of the further progress of absorption of the coagulum taking place under constant supervision, as from the size of the sac the risk of suppuration was deemed a possible contingency. During the whole period while the compression was maintained, iodide of potassium, in drachm doses, was given three times a day, without inducing coryza or other signs of iodic intoxication.

This is now the third case in which I have employed compression successfully in the treatment of aneurism. The two former cases I have already detailed to the Society, and in the remarks I then made I said that, so far from compression being an inferior means to ligature in the treatment of aneurism, it might possibly prove more efficient than ligature, by affording the means of interrupting the arterial supply through the main trunk at a higher or lower point according to the exigencies of the case, instead of being confined to one point, as was necessarily the case in the use of the ligature. The present case appeared to me to afford an opportunity of putting this to the test of practice. When I first saw this case, I pointed out to the pupils attending the visit that, as the anastomotica magna was given off just about the middle of the sac, the effect of any means employed to check the free circulation through the sac, which acted only upon the superficial femoral artery, but did not restrain the circulation through the profunda femoris, was not likely to afford such an interruption to the circulation as would suffice to effect a permanent obliteration of the sac cavity. I therefore, in the first instance, confined the compression to the superficial femoral artery, and was agreeably surprised to find that consolidation did occur. It did not, however, become permanent; for within forty-eight hours the aneurismal symptoms became established, not in the first instance by the superficial femoral becoming again pervious, but by the collateral circulation through the anastomotica magna bringing the blood round to the middle of the sac with sufficient force to break up the recent clot. It is quite possible that, at this stage, had I supported the superficial femoral by compression, so as to prevent the coagulum in it becoming disturbed, these aneurismal symptoms might have abated, just as we see in cases where the ligature is applied, the circulation in the sac is first stilled, then reproduced, and then gradually becomes completely and permanently arrested; the great difference between the two conditions of ligature and compression consisting in the permanent obstruction of the afferent artery at the point of deligation in the former, which does not necessarily occur at the point of compression in the latter. There is, however, a sufficient number of cases upon record to prove, after the treatment by ligature, that the circulation through the sac may become re-established, and the aneurism show no tendency to undergo any definitive consolidation.

If such a result occurs after ligature of the superficial femoral, flexion of the limb, ligature of the external iliac, or compression of the common femoral have been the measures proposed, should bandaging of the limb, the application of cold, or graduated compression of the sac prove insufficient. But flexion of the limb, in such a case as the present, could exercise but little influence upon the sac, as the disease was too high up to be directly influenced by the pressure, and bulged too much to admit of such flexion as could be assumed materially to impede the flow of blood through the popliteal below the sac.

The patient could hardly regard a second operation practised upon his limb for aneurismal disease with much satisfaction after, and because of the failure of the first; while to employ compression as a curative agency after the failure of the ligature would scarcely redound to the credit of the ligature, or be likely to lead the surgeon, who fell back upon it in a time of need, to speak of the ligature or make use of it so confidently upon a future occasion. Having made my experiment, and observed as its result that mere compression of the superficial femoral, however absolute, is not sufficient to effect a permanent cure of the disease where the aneurism corresponds to the origin of the anastomotica magna; furthermore, the aneurismal symptoms being now again reproduced in their original condition and intensity, I resorted to compression of the common femoral above the origin of the profunda, and found not only that the pressure was better borne because almost painless, but effected a thorough and complete consolidation of the sac some time within forty-eight hours after it was applied. The compression was so unflinchingly borne, and its application was so efficient in arresting all appreciable flow through the sac, that no aneurismal sign was observed, nor was there anything except the consolidated feeling of the sac, and the sensation of burning pain in the sac and leg, which could have indicated that all was silenced in the aneurism, till the withdrawal of the apparatus proved that it was really so. If we assume that the burning pain in the sac and limb beyond is pathognomonic of completed consolidation, then the compression was maintained for forty-two hours longer than was necessary, and these six hours formed the whole obligatory period during which it must needs have been maintained. The immunity from pain during this longer period indicates how easily, so far as the patient is concerned, pressure in this situation or a little higher up may be persistently maintained without the patience of either patient or attendants becoming exhausted. From what I have previously observed, I should expect the attendants to be more likely to give in than the patient; for while he may sleep, and generally does sleep, the watcher for the time being must be on the alert to prevent any movement of the limb or trunk from displacing the accuracy of the adjustment, and thus impairing the efficiency of the pressure upon the afferent trunk; for to compress the vessel leading to an aneurism efficiently the pressure must be sufficient to prevent the smallest wave of blood from passing beneath its control; it must, in other words, be *absolute*. If it is not, it is merely annoying the patient and losing time in obtaining a cure. How much time may thus be employed without effect is well shown in a case narrated by Mr Digby Longfield, of the Royal Navy, in a recent number of the *Lancet*,[1] where, after a month unavailingly expended in flexion, the use of Carter's compressor, Signormi's tourniquet, and digital compression, a brief application of the weight by means of a very inefficient machine sufficed to attain a satisfactory result.



*Author's Aneurism Compressor.*—A, Imaginary outline of limb. B, 12 lb. weight. C C, Metal arc, hinged at E, and opening by a pivot nut at D. G, Pinch screw of universal joint (ball and socket). I H, Upper part of rod, gliding through ball and socket, supporting and guiding the pressure weight. F, Splint to which the arc is attached, and on which the limb is laid. This apparatus was constructed out of a Carter's apparatus, by elevating the arc, removing the elastic, and elongating the rod as a support for the channelled weight.

I have not been able to discover any means of applying the weight more satisfactorily than the apparatus I formerly exhibited to the Society, and which I have employed in all those cases.

[1] Lancet, 18th December 1869, p. 838.

It admits of easy and accurate adjustment. It can be easily made by any mechanician, however rude. It utilizes all the weight necessary to arrest the flow of blood, but acts simply by dead pressure, there being neither screw pressure nor elastic bands in its composition. It admits of the pressure being regulated by the inclination of the axis of the weight. The form of the weight admits of its being easily borne without galling or unnecessary diffusion. Lastly, its metallic resisting surface secures the parts against all softening by soakage, or loss of power after it is once applied by yielding.

It has but one objection, viz., that it is not intelligent, and needs watching; but that is an objection to all compressors hitherto invented, and one which is not likely to be overcome so long as patients are restless and the compression must be maintained absolutely and exactly. With a little intelligence employed in its original application to the part, and in the arrangement of the patient's posture, this watching is reduced to a minimum, and the success of the result is likely to attain its maximum of efficiency.

# Part Second.

## REVIEWS.

*Psychologie Naturelle: Etude sur les Facultés Intellectuelles et Morales dans leur Etat Normal et dans leur Manifestations Anomales chez les Alienes et chez les Criminels.* Par Prosper Despine, Docteur en Médecine. Tomes III. Paris: 1868.

Here is a large and valuable contribution to the study of mental science, touching upon the great themes of mental alienation, moral evil, and man's responsibility. This is an old subject, which from the time of "Paradise Lost," and especially after the first great tragic act in the first human family, has troubled every thinking mind.

The title of this work is a striking one, and, although not new to psychologists, involves in the latter part of it a classification of lunatics and criminals by no means commonly observed or admitted. And yet the anomalous and abnormal conditions of mind under insanity and crime have a strange affinity with each other. Indeed, the purpose of this work we have shortly to notice, is to show, that crime is, in its highest forms, a disease of man's moral nature; that the "Sens moral" is an essential part of our mental constitution in its normal state; that the entire absence of it is found in all high-class criminals; and that high crime is "moral insanity." Without going so far as this, long years of the study of criminals, and intimate medical knowledge of them, have assured us that they are a marked variety of men—a most interesting study for psychologists; and these black sheep are as much a vexatious puzzle as the black variety of the human family to ethnologists.

This moral insanity is no new idea, and was often disputed among the classic philosophers, and the very method and principles of M. Despine were set forth in the groves of the Academy,

> "Where Wisdom talked with her Athenian sons."

Plato has been sneered at by our moderns for confounding crime and madness, and yet in reading M. Despine's work you almost have his *ipsissima verba*. Hear some of the sentences in which the divine Plato set forth his doctrines. "It may almost be asserted that all intemperance in any kind of pleasure, and all disgraceful conduct, is not properly blamed as the consequences of voluntary guilt. For no one is voluntarily bad; but he who is depraved becomes so through a certain habit of body, and an ill-governed education. All the vicious are vicious through two most *involuntary* causes, which we always ascribe rather to the planters than the things planted, and to the trainers rather than those trained;" and again, "There are two kinds of madness; one arising from human diseases, the other from an *inspired* deviation from established custom." Moreover, what a prophetic insight Plato showed, when he asserted that "the diseases of the soul result from the habits of the body." This physico-psychological belief, and these views of the alliance of crime and insanity, now slowly adopted in this country, prove Plato to have been wise in mental science.

We do not find our author giving much credit to his own countrymen for their attempts to describe that variety of insanity in which the intellect remains intact, and the moral sense defective. Among the French psychologists, Pinel especially deserves the best, for having indicated this form of mental disease. Pinel declared, one of the great objects of his life should be to demonstrate to the judges of the land that numbers of persons brought before them, made culpable and convicted as criminals, were but madmen. This was a cherished and strongly-pronounced belief of this great student of mind; and not a few of his countrymen have followed in his wake. Pritchard, too, in our own country, adopted similar views, and pointed out a form of mania, without intellectual derangement, which he termed moral insanity. These, and other labourers in this field, get scant justice done them in M. Despine's work, which is written to illustrate the phases of the criminal mind —in which he finds an entire absence of what he calls "sens moral." This is a most solemn and grave topic; and has never before received such an elaborate, learned, and able contribution as that before us. The zeal and industry displayed here deserve the highest praise; and the subject demands the honest and unprejudiced consideration of all philanthropists, moralists, legislators, as well as medical physiologists and psychologists. The book is written in a perspicuous and elegant style, and the method and arrangement are good.

The author has qualified himself for the study he now offers, by examining the records of crime, on a large and varied scale, in the collection of the *Gazette de Tribunaux*, since 1825 up to this time; and by every means in his power he has examined the subject. We therefore accept gratefully the substantial result of his labours in behalf of mental science, in three large volumes.

We have time to do little more in this review than give a bird's-eye view of this "Psychologie Naturelle," which is divided by M. Despine into two parts—*1st*, The study of the human mind in its normal state; and, *2dly*, In its abnormal states: the anomalies of insanity and crime. The title is justified in being a study of human nature; and belonging strictly to the domain of observation and the natural sciences. The definition of man, as given in the Catechism of the Catholic Church, "Man is a reasonable being, composed of a body and a soul, made after the image of God," is not satisfactory to the author, who prefers the following, which we give in his own words—"L'homme est un animal d'une espèce superieure, composé d'un corps et d'une âme; animal que rendent raisonnable et moralement libre les connaisances qu'il acquiert par les facultés morales et par les facultés intellectuelles." This definition will be seen to accord with his analysis of the mental faculties.

The mental faculties are divided into two kinds: Intellectual and Moral; the faculties of the intellect being three in number, viz.:—Perception, which receives through the medium of the senses knowledge of the external world; Memory, which retains that acquired knowledge. These two are termed an inferior order of faculties. The last is the Reflective faculty: the faculty of thought, of reflection, of searching after and arranging the ideas: the superior faculty, whose highest expression is Reason. To the moral faculties is given the name of Instinctive faculties, because they manifest themselves spontaneously, instinctively, in the natural aspirations and wants of the soul. The word *instinctive* faculties being used in the same meaning as *instinct* applied to the wants of the body; so the *instinctive* faculties relate to the wants of the mind. The instinctive faculties include the moral qualities expressed by the "sens moral," and these *instinctive* qualities are held by M. Despine to be an essential part of the normal state of the human mind. These views are by no means new, and in ancient and modern times are met with, but undoubtedly have been much neglected. In propounding his method and principles, M. Despine pays much respect to the Scottish school of philosophy, and he speaks despitefully of all metaphysics as breeding what Reid terms "metaphysical mania." But we rather think that mental science owes much to the speculative thought, even of metaphysicians. Plato himself, we have seen, by metaphysics worked out his system, so like to the one now set forth; and, as Coleridge says well, "Plato and Bacon had this only difference—the one more especially cultivated metaphysics, and the latter cultivated natural philosophy. Plato treated principally of truth, as manifested in the world of intellect; Bacon of the same truth in the world of sense; they both cultivated alike the science of method; and so we boast to have solved the great problem of conciliating ancient and modern philosophy." M. Despine seems to ignore Plato and the metaphysicians, and traces all progress to the Baconian method of observation of facts and inductive reasoning, superseding altogether hypothesis and analogy, and thereby advancing all the physical sciences. The science of mind alone, he says, has not had the benefit of Bacon's inductive system. But great honour is paid to the Scottish school of philosophy for applying these true principles to the study of the psychical faculties. To Hutcheson, Reid, and Dugald Stewart are given due praise for advancing psychology beyond all philosophers since their day, by bringing this study out of the uncertain region of speculation.

The present work does not profess to be a treatise on psychology: its aim being to rectify several errors; to enrich the field with some new discoveries; to bring under review former acquisitions; and to introduce mental science within the domain of the exact sciences.

The first part of this study is only introductory to the work, yet it occupies one whole volume under the head of "Normal Psychologie," a study of the intellectual and moral faculties, of reason and free will, or moral liberty. We have chapters on the Intellectual Faculties; on the Instinctive or Moral Faculties or the Sentiments; on the law which presides over the Reflective Faculties during the manifestations of the Instinctive Faculties; on the "sens moral," or moral faculty—a psychological study of the passions; of Reason, of Free Will, and of the Functions of the Nervous System. We can do little more than merely give this syllabus of the contents of the first part. We must not, however, omit noticing what much stress is thrown upon, viz., the formulating of the following law as of great simplicity, but fitted to solve a great many mental and moral problems hitherto unsettled in psychology. This law is, "that the reflective faculties act simultaneously with the instinctive faculties." As there are two orders of psychical faculties—the intellectual and the moral—there are two kinds of reason, the intellectual reason and the moral reason. The former recognises the objects of nature, teaching man the true and the intelligent; the second is cognisant of moral truth, and makes man responsible for his acts.

Here psychologists are much blamed for neglecting the duty of studying mental and moral manifestations in different human races—in the varieties of our own race—in cases of eccentricity, irrational and criminal,—all which he insists upon as affording fruitful sources of new information in mental science.

The following startling conclusions arrived at in regard to the criminal classes are announced and pressed strongly upon the reader in various ways. After long and laborious study of the records of crime and the history of criminals, two characteristics are given as invariably found to belong to great criminals:—

1st, *The absence of moral reprobation before committing premeditated crime.*

2d, *The absence of remorse after the committal of the act.*

This utter absence of moral sensibility infers the question and answer as to moral responsibility. Are the acts of habitual or great criminals done involuntarily? Are the acts of criminals done by an irresistible proclivity to crime? Is the absence of moral reprobation before and of remorse after the acts, *moral insanity?*

Familiar as we have been with crime and all kinds of criminals for many years these propositions appeared to be set forth in terms by far too absolute. Yet, on careful thought on this momentous matter, almost the same results as this writer has come to coincided with our experiences, which, perhaps, are second to none in Scotland, so far as the study of criminals can be made from a medical point of view. The testimony of governors, chaplains, matrons, and officials in prisons, concurs in the following statements:—

Of thousands of habitual thieves, we have never known in a single case any remorse. A gentleman, forty years among criminals here and in the colonies, told us a few days ago that he never knew one habitual thief reformed. Can any stronger proof be given of the absence of remorse? Of the criminal lunatics of Scotland who have committed murder, and become sane, we could never learn but of one who ever showed contrition. This poor fellow killed his father, mother, and aunt in one night by cutting their throats. He has had years of lucidity, during which he is subject at times to the most painful and agonizing grief; engaged with his Bible, he gives himself up to tears, and groans, and prayers in secret.

Another class of murderers, the dipsomaniacs, who have been sane from the date of the offence, several of whom have been five to seven years in prison, have never been known by any official to express sorrow for the loss of parent or wife or child, or regret and penitence for their sinful deeds. Another class—the females who have committed infanticide, numbering about 150—with only two exceptions, have never been seen or heard to express remorse. These observations are extended to twelve years' daily intimacy of medical superintendence over all these classes of criminals. But these remarks apply only to remorse *known*:—as to the absolute want of the moral sense reproving criminals before and after their crimes; it must not be concluded, because it is not known, that there does not exist any "sens moral." As there are few physical monsters, we cannot think but that *moral monsters* must also be rare. The conscience in various degrees may be weak as well as the intellect; and as between mere functional derangement of the body and organic disease the difference is great, so by analogy we should look for a weakness of the moral sense through many stages before we pronounce on a case of "moral insanity." We think Despine's statement too strong every way. Our great dramatist, who knew human nature perhaps beyond and better than any man, gives us ample scenes of moral reprobation before the murderer could "bend up each corporal agent to the terrible feat;" and it is of the "compunctious visitings of nature" that Shakespeare speaks through the mouth of Brutus when contemplating the assassination of Cæsar:—

> "Since Cassius first did whet me against Cæsar
> I have not slept. . . . .
> *Between the acting of a dreadful thing*
> *And the first motion, the state of man,*
> *Like to a little kingdom, suffers then*
> *The nature of an insurrection.*"

Notwithstanding of their extreme defects of moral sensibility, although it is not seen upon criminals, it is felt and hidden; and although almost lost or asleep, we believe conscience is still there, and there is that torture within "which passeth show." Hence the occasional revelation of deeds no one knew but the actor in the tragedy; and hence the terrible dreams and distress which have disclosed murderers who had long years concealed themselves and their crimes.

One of the most interesting chapters of the first part of the study has reference to the hereditary transmission of the instinctive or moral faculties. The experience of M. Despine clearly points to the congenital cause of moral perverseness and crime—the defective organization of the brain and nervous system. He says, "Heredity is one of the ways in which by natural laws our faculties are transmitted." He refers to the great authority of M. Prosper Lucas in his learned treatise on Natural Inheritance. No physiologist nowadays, we think, denies the doctrine as a general law (to which there are exceptions no doubt) that fathers transmit to their children hereditary vices and virtues. If so, says Despine, where is the moral liberty? In the criminal classes this hereditary nature seems very marked, and is becoming much more so in this country since transportation was abolished. The power of the law of heredity is undoubted, and it will not do to cry out against it by the charge of atheism, materialism, and infidelity. "What!" exclaims this writer, "shall we refuse to study the natural laws—nature herself? The study of such is to study the works and ways of the great First Cause, and the wisdom that made these natural laws."

Here we note, that although M. Despine is, we think, of a speculative character of mind, and devotes too much of his work to this indulgence, yet he comes down in the greater part of the work to the serener and surer region of experience and fact. He likes to indulge in questions of free will and moral responsibility, which puzzle men in all ages, as did the angelical host, whom Milton describes, as they—

> "Sat on a hill retired—and reasoned high
> Of Providence, foreknowledge, will and fate,
> Fixed fate, free will, foreknowledge absolute,
> And found no end in wandering mazes lost."

But he is a physico-psychologist like the greatest of the French school of mental science. He traces the mental state, normal and abnormal, to organization.

M. Despine belongs to the school of Morel, Moreau, Prosper Lucas, and other distinguished physicists of his own countrymen. He refers the "sens moral" to transmission from parents to children, as Zacchius gives it, in the following terms:—"Il est certain que la plupart des penchants et des affections de l'âme naissant de la semence de parents, ainsi que le corps." So also Jacob Boehme, after much doubt and thought, concluded the same, and said, "The soul is propagated hereditarily through the material physique." Looking at the intimate relations of mind and matter, what an important and serious question becomes the moral responsibility of the caste of criminals!

We hasten on to the remainder and more valuable part of the study.

The second part of this work is entitled—"Psychologie Anomale," or a study of insanity.

The subjects treated of are—Hallucinations and Illusions; the different forms of Mental Disease.

And the rest of the volumes, amounting to about two-thirds of the whole, is devoted to—Moral Madness, characterized by the absence of the moral sense, and of the other moral sentiments opposed to immoral desires. This is a psychological study of criminals.

The first chapter has for its title—"The Mental Characteristics of Criminals." Here we have a repetition of the two grand characteristics upon which so much stress is laid, viz., The absence of moral remonstrance before, and the absence of remorse after the crime committed.

It is quite clear that M. Despine, although he has studied the records of crime, has seen very little of the criminal classes, otherwise he would have noted under this head the extreme mental weakness belonging to them as a class. It is quite a vulgar opinion, never entertained by any experienced observer of criminals, that they are a clever and accomplished class of men. They are low in intellect, cunning, and seemingly unfit for almost any acquirement except their mean arts of duplicity and sleight-of-hand. In mental calibre they are far below the classes of industrial operatives. Neither does he note the *physique* peculiar to them indicative of a low caste of men, quite inferior to the common industrial classes. The physical is doubtless the indication—the foundation of the moral man. We quite agree, however, in regard to his inquiry how far moral freedom may be allowed to belong to the more degraded criminal class, although we dissent from the absolute dictum that all great criminals are morally insane.

We shall try to give some distinct notion of the views of M. Despine, which are met with in this volume.

Having treated of the faculties of the mind which belong to man —reason and moral freedom—he completes his study by describing the mental states in which man possesses neither reason nor moral freedom. This study is insanity. The term madness has been generally applied to those anomalous states of mind where individuals suffer from chronic disease of the brain; this madness is everywhere regarded as a disease, and every man who has not disease of the brain is supposed to be reasonable, intelligent, and a free agent. This opinion M. Despine looks upon as quite erroneous. We meet, says he, with instances of insanity, undoubted insanity, where there is thoroughly awanting free agency, and where there can be no responsibility, among persons in sound health as well as among those labouring under disease. To the psychologist, an insane condition of the reason and intellect is not a disease; it is a state of mind marked by its incompatibility with moral reason and free agency. The analysis of this mental state in different individuals who imagine and reason contrary to reason—that is to say, to truth and morality, without being guided by reason—has convinced him that this mental state is identical in all, and only varies in form, as their brain is sound or diseased.

This state consists, says M. Despine,—

1*st*, In having ideas, false, absurd, immoral, inspired by the passions either natural to the individual or induced by a diseased condition of the brain: or,

2*dly*, In not being able to detect the falsity, absurdity—immorality of the passions, those thoughts and desires which they inspire, because these passions entirely control the mind, there being an utter absence of those rational and moral sentiments which are opposed to them, and which can alone make known the nature of truth and morality. Is not this the condition the far-seen dramatist describes as existing in Macbeth before the murder of Duncan, when the reason struggles with the inward monitor, and "prudential reasons" from the judgment fail to overcome the wicked passions of the soul? This mental state, he continues, is exactly the same when the rational and moral sentiments are stifled in the mind by the passions as when the individual is not possessed of the rational sentiments. In the one as in the other case, man can have no cognizance of the false, bizarre, perverse nature of the thoughts and desires inspired by his passions. In both cases, man cannot have the desire and the will, and consequently the power, to combat his thoughts and desires, having no means withal to combat them.

The distinction betwixt the madness with disease and madness with health is this: with disease, it is the brain disease which makes the passions rage and rule the mind, and so stifle the reason; whilst, in the healthy state, the passions are given to the sound brain, and are natural to the character. But in the one case as in the other, the errors, the *bizarreries*, the perversities which the passions inspire, are not the chief element in the madness, they are only its object. The chief element is the state of passion—the absence of any *instinctive* reason to the irrational and perverse desires or appetites—it is the impossibility in which the victim finds himself to appreciate the nature of the thoughts and desires—it is the impossibility in which he finds himself to resist them, because there is nothing in his moral sense to do so.

The following passage gives M. Despine's opinion of the moral state of the criminal mind:—

"The madness of criminals is that of individuals who, possessed by instinctive perverse elements, are without any moral sentiments opposed to these instinctive elements, and without the moral sense. Inquirers have carefully fixed their attention upon the psychical state of the insane under disease, but no one has occupied his mind with that of criminals, all having considered the state of the latter as altogether normal. Nothing is further from the truth. This work shall demonstrate the reality of their psychical anomaly—an anomaly distinctly characterized, natural and involuntary, as statistics show to be reproduced in human nature with unvarying regularity."

We have sundry chapters radiating naturally from the great central fact of moral insensibility, of the perversity, and of the imprudence and want of foresight among criminals, of which ample evidence is everywhere seen. Not to extend this inquiry to the higher criminals, we have these peculiarities and weaknesses very notable in the common thieving class, seen in the large percentage of reconvictions. A gentleman, who for forty years has been in continued and extensive charge of criminals, says, that he never saw a single person of the thieving class proper, cured by all the imprisonment discipline to which he was subjected; these imprisonments being of an average from fifteen to thirty convictions, often to fifty times. Certain families, for several generations, are supported at the public expense in prisons the greater part of their lives.

We have a very interesting article under the same head, "Of the relàtion betwixt Crime and Pathological Insanity or Brain Disease;" and there can be little doubt that, as a cause as well as a consequence of crime, insanity in various forms belongs to criminals in excess above any known community. The statistics of criminals lapsing into insanity in the prisons of this country, as well as on the Continent, are quite unparalleled in the excess of their proportional numbers. The habitual criminals of the hereditary class who have shown an irrepressible tendency to crime, go out and into prison from their earliest years, by-and-by become weak-minded, and at length sink into imbecility, or break out into furious mania. All prison blue-books tell a similar tale of mental and moral imbecility.

We have, in the third volume, a psychological study of the persons who commit infanticide, in which the causes are treated of with great skill and research, and full of medico-legal information: of seduction; of the causes of infanticide in persons of moral character; of free agency in such persons; of infanticides committed by women who possess *le sens moral* and the opposite, etc., etc. We have homicides and suicides with their causes, and the condition of mind leading thereto, such as despair or ennui, dissipation, religious passion, injured pride, self-esteem, etc., etc. We have next a chapter on incendiaries, robbers, thieves, ending with an excellent summary of the subject of prostitution, which has been so practically studied by the surgeons of France above all other nations.

The work of M. Despine ends with a review of the various kinds of treatment of criminals, and M. Despine's own opinions upon these and upon the preventive treatment of crime. Every chapter on the different forms of crime is full of information bearing on medical jurisprudence. We have to find fault with the author for not tabulating his cases, with particulars. Perhaps he may think of this in his after experiences. When an individual case is given in proof, it is easy to oppose it by an exceptional one. The Registrar-General gives a table, proving on a large scale the excess of female births over males; and it is only absurd for Mrs Grundy to oppose it by saying she and Mrs Harris had only a son and a daughter, and therefore the sexes are equal. We desiderate statistical tables.

After recording at great length the case of Lemaire, and several moral monsters, the second volume of this singular book concludes thus: "How, in the presence of facts like these, and of others equally conclusive, can we suppose, with philosophers, that great crimes are the production of perversity alone, and that those who commit these revolting acts are in the possession of the moral sentiments common to other men? How can we fail to recognise in these unfortunates a natural moral idiocy, a monstrosity in morals, and their moral aberrations fired by their bad passions, their consciences asleep, false, opposed to the higher human sentiments? Nowhere the moral conscience exists. Nature does not allow it to sleep or deceive, in the presence of thoughts repugnant, and callously cherished. Far from this, she reacts against bad thoughts with horror and indignation. A crime committed in cold blood is not explicable but by the insensibility of the moral nature, which deprives a human being of moral freedom, and can be imputed to perversity or perversion only among individuals endowed with the moral sense."

We have thus hastily glanced at M. Despine's work, which conveys, in rather strong language, his conclusions as to the moral insanity of great criminals. At the same time, we are pleased to see criminals brought before us as an anomalous class of men, whom we must admit to be bordering upon mental imbecility. It must always, however, be a question of fact, not easily settled even by experts, at what period of his career the criminal has lapsed into moral insanity, and becomes irresponsible to the laws he is persistently violating. We have long been of opinion that the work of prison-surgeons, and the study of what is to be done with our criminals, is properly within the domain of psychology. That the instinctive or moral faculties are an essential part of our mental constitution; that the criminal class, beyond all others, are of low moral sensibility; that they are in mental, as well in moral, endowments much beneath the social industrial classes; that they are liable more than any other community we know to become insane, and suffer from cerebral disease. These characteristics we admit, in common with the zealous and able writer whose works we have been examining.

Chapter X. is given to the moral treatment which should be applied to criminals and delinquents. In this we find little new, but a defence of the present amenities and ameliorations of prison discipline. He quotes Michelet at the outset: "La science et la science de la nature sont une. Il faut que la justice devienne une médecine s'eclairant des sciences psychologiques." If the premise as to criminals is true, that there is an entire or even a partial absence of the moral sense and moral freedom, then the conclusion is inevitable, that punitive treatment is unjust and sure to prove injurious. M. Despine holds that there is no remedy for a moral disease but the moral remedy of cultivation of what he calls the instinctive faculties. But if the instinctive faculties are not there to cultivate, then we have a painful and unsatisfactory task. No wonder the treatment defies the collective wisdom of teachers, preachers, people, and parliaments.

The very weak-minded and imbecile state of the greatest proportion of prisoners has long since led to the abandonment of the silent and the solitary system, and in a great measure of the cellular or separate system of imprisonment. Even with all the modern ameliorations of the associated system, it is too true that the amount of insanity from brain disease is in excess beyond any comparison in any nation or social community of which we have a statistical information. The separate cellular system in its mildest form is not easily borne by such weak-minded persons as criminals.

Sir Archibald Alison, in his "History of Europe," writing on crime, gives the result of a long life and a large experience in these remarkable words:—"It will probably be found in the end by general observation, what no small experience in these matters has convinced me of, that vice, in the classes where it is in a manner hereditary, is incapable of reformation by any length of solitary confinement at home." He recommends transportation.

The present reformatory and penitential system is the one approved of by M. Despine, in which the only punitive element really is the seclusion from the world of social and civil society. It is no part of discipline to injure either the body or mind of the prisoner;

and it seems necessary, in spite of all that is talked of by the ignorant of "prison pets," and the hygienic system adopted to preserve their psychical and physical condition, that good diet, exercise, and other amenities, should be maintained. And, in spite of all, insanity in various forms is the chief business of prison surgeons; insanity, no doubt, having its *point du départ* in the immoral habits, hereditary taints, and dissipation, as well as frequent confinements in prisons,—all which may be traced in general to congenital defect of the moral and instinctive faculties of which we have been speaking. Physical suffering, we quite agree with our author, should have (as it has) nothing to do with pœnology or the science of punishments. As Cicero long since said, "Omnis et animadversio et castigatio contumelia vacare debet. Prohibenda autem maxima est ira in puniendo." Quite in the same spirit, and the true one, says Despine, "The cure is not by discipline through chastisements, nor by hard labour, nor by any instruction, except such as can modify the instinctive nature of the bad individual into moral feeling by the cultivation of the moral sentiments."

This valuable work we have been glancing over confirms all the opinions we have long held, and which daily experience strengthens—that the mental and moral nature of confirmed and great criminals is in many cases moral insanity. The frequent re-committals of the common thieving class, the irresistible tendency to the same acts, the hopelessness of their reformation or improvement by penal or moral treatment, go to prove the criminal class are under a moral disease. Several years ago, Mr Simpson of Edinburgh proposed the plan of making a prison a kind of moral hospital to cure incorrigible offenders. But who can fix when the cure is completed? The worst behave often best in prison; but if a criminal has been ten or twenty times convicted, is he *not* curable? If this be proved, why, then, our prisons must be asylums for the morally insane, where they may be kept under proper care and treatment as dangerous persons to be at large. It does appear, in regard to advanced and great criminals, that all the array of chaplains, Scripture-readers, and teachers, secular and sacred, and strict habits of order, decency, and sobriety for long years, do no good. Why, then, let society save itself from the expenses and the dangers attending the present system. But how? Certainly not as some would have it, by the old system of attempting to whip out the offending Adam, by the severities, the starvation, the jail fevers, and the cruelties,—from which we were saved by the benevolent exertions of Howard, and the legal amenities of Romilly,—but by long and almost life detentions in our prisons as moral asylums. Much has been said and done of late in the stamping out of physical diseases. Why not apply it to the great moral epidemics that ravage social and civil life? Not by incisive acts of injury against persons, many of whom are certainly irresponsible; but by the system of isolation of the infected from healthier members of the body social. Here I venture to quote from the work of a man as experienced and zealous in the study of the criminal as any one I could name—Mr Frederick Hill, late Inspector of Prisons:—"According to the evidence collected by the Poor-Law Commissioners, crime often proceeds from father to son in a long line of succession. This circumstance is favourable for its gradual extinction. Indeed, if all the criminals in the present generation could be collected and confined, the young to be cured, and the old to pass their days under control, the next generation would probably contain but few thieves. One of the most serious evils perhaps of the system of short confinements is, that it allows the perpetuation of a race of criminals." And believing, as we do, that the criminal class is low in caste, and many of them morally imbecile, fast tending to moral insanity, we cannot think this proposition cruel even in respect to the criminal. Hale had an axiom, "That all criminals are insane." He had another saying, "That while pity is due to the criminal, pity is also due to the country." What innumerable evils might be prevented by long or life confinements of the criminal! how many millions would be thus economized to the country!

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## *On the General Principles which should be observed in the Construction of Hospitals.* By DOUGLAS GALTON, C.B., F.R.S., late Captain Royal Engineers.

THIS work consists of an Address delivered to the British Medical Association at Leeds in July last, the author's attention having been previously directed to the subject, more particularly as one of the Commissioners for the Improvement of Barracks and Hospitals, appointed in 1858.

The author treats chiefly of the principles upon which hospitals should be constructed; but he also enters very fully into such details as are important, because it is by attention to these that a proper and efficient hospital is made up.

The definition here given of an hospital or infirmary is, "A building intended for the reception and treatment of sick and injured persons, under conditions more favourable for recovery than such persons could otherwise command." Also, "that it should enable the sick to recover in the shortest possible time." The author then lays down those requirements which are to be kept in view, consisting of—1. Pure air; 2. The means of supplying warmth; 3. Pure water; and, 4. The most perfect cleanliness within and around the building.

How to realize these requirements is the great object which the author then proceeds to explain; and this he does under the following heads:—the selection of a site,—the form of the rooms or wards,—their distribution and other necessary accessories, which when combined form the hospital.

*Site.*—This should be in a healthy locality, free from nuisances or marshy ground, in an open country, upon porous and dry soil, sheltered from the north and east, with sufficient natural drainage outlets, and a plentiful supply of water. The clear area should afford ample space for the hospital,—this being about an acre for every 100 patients.

*Wards.*—As these form the basis and chief part of an hospital, they must be so constructed as to secure the free circulation of air, sufficient size, easy access, and facility of nursing and discipline; from twenty to thirty-two beds being the number considered best for the largest wards, and sufficient for one nurse to watch over by night or by day. Smaller wards are also necessary for isolating bad cases.

*Purity of Air in Wards.*—Pure air, when confined within an inhabited space, becoming necessarily vitiated by respiration and emanations from sick persons—and more especially from those affected with fevers, wounds, or sores, being of a poisonous quality—requires constant change by ventilation. The larger the wards, the more easily will this be effected without altering the temperature, or causing injurious draughts. It is advantageous that the ventilation of each ward should be kept distinct and independent of others. It must possess ample means for the purpose—efficiently maintained and regulated by a responsible party. The means considered best seem to consist in open fireplaces, gas sunlights, and common sash windows. The author also remarks, that "mechanical ventilating arrangements are always to some extent more or less affected by variations in the temperature of the outer air and by the direction of the wind, and consequently require careful and constant supervision. Practically, hospitals dependent upon such means alone for ventilation have not been healthy. It may be that the process is not in accordance of *nature's mode* of providing fresh air."

*Area or Space required for each Bed.*—The superficial or floor space for each bed varies in different hospitals from 80 to 112 square feet. About 90 square feet is considered a sufficient space; but in fever and lying-in wards more should be allowed. There should be one window at least between every two beds; and 7½ feet for each bed along the side wall, in a ward 24 or 26 feet in breadth and 14 feet in height, would afford ample space for all purposes, and about 1200 cubic feet of air space for each patient.

These being the most important parts of detail, we must pass over the minutiæ of other subordinate accessories, fully treated of by the author, and well deserving of attention.

*Aggregation of Wards, etc., in the construction of an Hospital.*—The author considers "the ward with its ward offices to be the unit or basis of hospital construction;" and adds, "that it is a small hospital, which may be increased to any required size by the addition of similar units." The conditions upon which these should be added together are—1. "There should be free circulation of air between the pavilions." 2. "The space between the pavilions should be exposed to sunshine, and the sunshine should fall on the windows." 3. "The distance between adjacent pavilions should not be less than twice the height of the pavilion, reckoned from the floors of the ground-floor ward." 4. "The arrangement of pavilions should be such as to allow of convenient covered communication between the wards, without interfering with the light and ventilation; and therefore the top of the covered corridor uniting the ends of pavilions should not be carried above the ceiling of the ground-floor ward." 5. "No ward should be so placed as to form a passage-room to other wards." 6. "As a general rule, there should not be more than two floors of wards in a pavilion." 7. "There is a limit to the numbers which should be congregated under one roof. This limit will depend very much on the nature of the cases." Not more than 136 cases should be placed in one double pavilion, divided into two equal halves, cutting off through ventilation. Any increase of beds required should not be by enlarging the wards, but by additional pavilions.

Day-rooms should be provided adjoining the wards for convalescent patients, where there are no convalescent institutions.

All corridors connecting the pavilions or wards should be kept low, with windows on both sides for free ventilation. The staircases should be wide, and supplied with windows for the free circulation of fresh air.

*Administrative Buildings.*—Apartments for the house-surgeon, matron, nurses, and servants, with other necessary accommodations, should be situated as much as possible apart from the wards. The kitchen, store-rooms, laundry, and sculleries should be exterior to the hospital.

In applying the above principles to existing hospitals, the author illustrates the subject by pointing out the defects of several well-known hospitals from inattention and deviations from them, such as the King's College Hospital, the Royal Victoria Hospital, Netley, Queen Charlotte's Lying-in Hospital, and the Marine Hospital, Woolwich. Several important points, as defects in hospital construction, are also pointed out in the Lariboisière Hospital at Paris, and the new Leeds Hospital, upon which no exertion or expense have been spared to render them complete and perfect as model hospitals. In the one, the pavilions are too close to each other; and in the other, the glass covering of the interior court, by preventing the free circulation of air, are very objectionable.

*Conclusion.*—The author concludes by stating as his opinion that, although many hospitals have been constructed at much greater cost, the erection of an hospital for in-patients, with proper attention to economy, should not exceed from £90 to £120 per bed.

There should be no superfluous ornament, corners, or projections about hospital buildings, as they delay and stagnate the air, and form receptacles for dirt; as also incurring extra outlay and constant repairs.

"These various conditions," our author adds, "it is the part of the medical man to lay down. My part has been to endeavour to show how the architect must shape his building, so that it shall be in accordance with what the physician has declared to be necessary."

The very distinct, comprehensive, and useful short treatise, of which we have now given an analysis, we recommend to the attention of all those interested in hospital construction. It is the result of much study and experience of the subject, and of one whose opportunities of observation have been extensive. It is by no means so full of information as some of the treatises of other authors, but, so far as it goes, the opinions and statements of the author are valuable as confirming those of others, whose sentiments are deserving of attention.

The author has added notes of the speeches of several distinguished members of the Association who took part in the discussion which followed the reading of his address. These are laudatory and expressive of approbation of the author's statements and remarks; but we must leave these to the consideration of those who peruse the book.

The work before us contains an Appendix, describing ventilating fireplaces and grates, invented and employed in some of our barracks and hospitals.

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*Ueber die Schaedelform bei Gesichtslagen.* Von DR CARL HECKER. Berlin: 1869.

*On the Form of the Head in Face Presentations.* By DR CARL HECKER. Berlin: 1869.

HECKER had some time ago enunciated his views as to the influence of a certain shape of the fœtal head in causing a variation from the ordinary mechanism of parturition. Hecker's views Winckel has, with more zeal than prudence, opposed. But to Winckel's light criticism we owe the present little work of Hecker, who has wisely thought it worth while to reiterate his views, increase the evidence in their favour, and overthrow Winckel's allegations. We may now express our conviction, that Hecker's theory has nothing to fear from Winckel, that he has written an excellent essay, containing a notable contribution to the mechanism of parturition, establishing it by all the means of observation which have been within his reach.

Hecker's discovery is easily stated. It is, that the dolichocephalous condition of the fœtus accompanies to face presentation. This he shows, by pointing to the increased average antero-posterior length of the head in children born with the face presenting. Against this evidence it has been alleged that in face cases the dolichocephalous condition is produced by the face presentation; not *vice versa*, as Hecker supposes. This adverse criticism Hecker answers by pointing to the state of the heads in face cases after the lapse of weeks: they are still dolichocephalic, still in contrast with the average fœtal head or head of young child.

Hecker advances a step farther, pointing out that the dolichocephalic condition acts in early labour so as to produce face cases by the greater proportional length of the posterior arm of the lever formed by the long axis of the fœtal head moving on the atlas. It is well known that the Roederer obliquity of the fœtal head at or near the brim is accounted for by the greater length of the anterior lever arm. In the dolichocephalic condition this preponderance of the anterior arm is diminished, and extension of the head, as in face cases, is more liable to occur than otherwise it would be.

To every student this, well established as it is by the limited observations of Hecker, must be a cheering addition to our obstetrical knowledge. Knowing, further, as we do, that the transformation of a vertex into a face case is much more likely to happen when the occiput looks to the right than when it looks to the left, we seem to be getting a glimpse of a reliable theory of face cases.

# Part Third.

## MEETINGS OF SOCIETIES.

### MEDICO-CHIRURGICAL SOCIETY OF EDINBURGH.

### SESSION XLIX.—MEETING VI.

*Wednesday, 2d March* 1870.—Dr OMOND, *a former President*, in the Chair.

I. *Dr Joseph Bell* exhibited A PATIENT, aged 42, who had been for a considerable time under his care for extensive necrosis of the shaft of the tibia. The disease involved the cancellated texture of the head of the bone, as well as the shaft, and the joint was opened into by its ravages in the head. Acute suppuration of the knee-joint followed, and for some time amputation seemed almost inevitable. However, under careful treatment, including rest on a splint in the extended posture, free evacuation of all abscesses, and removal of numerous exfoliations, a cure was obtained. The limb is now absolutely straight, shortening exists to the extent of about a quarter of an inch, firm bony anchylosis of the knee-joint has been secured, and the patient walks strongly and firmly with a scarcely perceptible limp. With the exception of a deep depression in the position of the tuberosity of the tibia, and numerous and deep cicatrices of sinuses, the appearance and plumpness of the limb is little altered.

II. *Dr Joseph Bell* also exhibited an interesting preparation of A FRACTURE OF THE THIGH, which he had obtained from Dr Gillespie. The patient, a feeble old man, æt. 76, had sustained a simple fracture of the right thigh at the junction of the upper and middle thirds. He was admitted under Dr Gillespie's care in the Infirmary; great difficulty was experienced in replacing the fractured ends, and still greater in retaining them in position. Different apparatuses were tried, but all failed from the restlessness of the patient, and the excessive liability to sloughing of the integuments under the slightest pressure. He died exhausted by abscesses and bed-sores about eight weeks after admission. On examining the fracture, it was found that one small abscess existed, in which the broken ends lay rough and rounded off, but prevented from coming close to each other by a piece of dead bone, about one and a half inches in length, which lay right across their ends. This dead piece was already nearly separated from its soft parts. A clot was found in the iliac vein.

III. *Dr Chiene* exhibited A CALCULUS taken from the bladder of a female subject, aged 22, brought to the dissecting-rooms of the University. It was remarkable for its large size, measuring eight inches and a half in one circumference, and six inches and a quarter in the other; it was two inches and three-quarters long, two inches and a quarter broad, and two inches thick. It weighed six ounces of 437 grains each, and was composed of oxalate of lime, with a thick layer of phosphates externally.

IV. *Dr Chiene* also exhibited A LARGE PIECE OF BOILED MEAT removed from the pharynx of a beggar man, aged 60, who had died suddenly under the following circumstances:—

He had been in the habit of getting porridge and milk from a patroness; the last time he called she was engaged, and gave him instead some bread and beef; he left her eating the bread, and with the meat in his hand, and within one minute was found breathless and dead on the stair leading to the house. He had been ravenously hungry, having stated that he had not tasted any food since the previous morning, a period of full thirty-six hours. On inspection, his mouth was found full of bread and fat, and a piece of beef was found lying in the upper part of the pharynx, just over the glottis.

V. *Dr Gillespie, P.R.C.S.*, then read his paper entitled, NOTES OF AN EPIDEMIC OF TYPHOID FEVER AT DONALDSON'S HOSPITAL, which will appear in our next number. In answer to a query by Dr Gillespie,

*Mr Benjamin Bell* remarked that he never had a case of enteric fever in George Watson's Hospital, saving the solitary one whose peculiar history he was about to communicate to the Society. He asked Dr Gillespie if, in the peculiar case mentioned by him, there was nothing in the locomotion indicative of paralysis rather than weakness, and whether the singular abnormalities in the vision might not be explicable on the same theory.

*Dr Gillespie* said he had only seen the patient once out of bed, and though he had a staggering gait, it seemed to him to be entirely due to debility. There had been serous effusion into both pleuræ, great delirium, and a very tedious illness.

*Dr Argyll Robertson* remarked that, in all the acute exanthemata, especially in scarlet fever, but also in typhus and enteric fevers, temporary loss of sight was no infrequent occurrence, but in such cases there was no anormal condition visible with the ophthalmoscope, and the pupil still acted under the influence of light. These cases Von Graefe had explained by supposing that the retina still retained its sensibility and the optic nerve its force, but that the sensorium was incapable of conducting the impressions, or the intellect incapable of taking cognizance of them. In regard to the peculiar form of vision described, in which the boy saw perfectly, except what was presented in the middle line right in front of the eyes, the only feasible explanation could be that the outer half of each retina had somehow lost its power of seeing, and that the converging power had also been lost, the central part of the retina remaining healthy. It was possible that an ophthalmoscopic examination might have revealed whether this arose from an affection of the retina or was due to disease of some of the more central parts, such as partial affections of the optic tracts, of which many cases, depending on different morbid affections, were on record.

*Dr Grainger Stewart* remarked that the connexion traced by Dr Gillespie between the outbreak of the fever and the deficient trapping of the drains, as revealed by the existence of a bad smell, was extremely interesting; and reminded him of an outbreak of enteric fever which took place some years ago at Penicuik, when, in consequence of the lowness of the river Esk from drought, the fæcal matter conveyed to its bed by the house drains was suffered to accumulate and putrefy in its bed, the result being an outbreak of enteric fever, mainly confined to the houses situate on the banks of the stream.

*Dr Bennett* regretted that circumstances had prevented his being present sufficiently early to hear Dr Gillespie's paper, but with regard to the general question as to noxious gases from bad drains being the specific or essential cause of enteric fever, he considered it to be far from proved. From the prevalence of water-closets, and the fact that no one could draw up the handle of one of these without admitting a stream of foul air into the house, there was ample *a priori* evidence in favour of this view, which was, however, inconsistent with a large number of facts. In the years 1833 and 1837, when great epidemics of typhus alone prevailed in Edinburgh, there were as many noxious fumes prevalent in the city as now. When he went to Paris in 1837, he found only enteric fever prevail there, and typhus was so utterly unknown, that he could not get the Parisian physicians to comprehend it. Dr Lombard, of Geneva, made a medical pilgrimage to Edinburgh in order to study the disease. Water-closets and fæcal emanations abounded then as they do now, yet, whenever Dr John Reid, the pathologist to the Royal Infirmary, found ulceration of Peyer's patches in fever, he at once referred the origin of the case to Linlithgow or Bathgate, the only two districts of Scotland at that time affected with typhoid. Since then many instances had been published when an outbreak of enteric fever occurred in boarding-schools or communities among whom the water-closets were defective. But Dr Bennett held that there were emanations without epidemics, and epidemics without emanations. When in Naples in 1863, he found the city full of the vilest odours; the houses built on the height surrounding the bay had their drains running down to the sea in straight lines, with open gratings every here and there. The houses were built as ours, in flats, but, contrary to our custom, the rent rose as you ascended, the garrets being the dearest of all; partly, no doubt, for the sake of the view, but chiefly because you rose above the filthy smells. For half a mile out the inner margin of the bay was like turbid pea-soup; beyond that the brilliant blue of the sea became visible; and to add to these vile smells, there was a constant escape of sulphuretted hydrogen from the volcanic soil beneath. It might be supposed that here, if anywhere, enteric fever must be paramount. But what was actually the case? In the general hospital which he visited there were one thousand beds; in it the latrines were chambers at each end of the corridors, with a series of holes in the floors, into, or as nearly into which as possible, the fæces were allowed to fall. The ammoniacal exhalation from these apartments was something astounding; yet the reply, on inquiry, was that typhoid fever was trifling. Again, in the military hospital of the same city, with similiar arrangements and disgusting emanations, one case of fever was all that could be discovered among 800 beds. When he had mentioned these facts at the meeting of the British Association at Bath, Drs Livingstone and Kirk stated that the smell emanating from the mangrove swamps in South Africa was something dreadful; and that when forced to encamp in such places, fever was expected as a certain result, but it never occurred. Again, in the Campagna around Rome, in autumn, fever prevails to a great extent; but there we have no smells, neither are there any at Rio Janeiro, where yellow fever prevails, nor on the pestiferous coast of Guinea. When Prince Albert died, it was endeavoured to be shown that Windsor Castle was badly drained; nothing, perhaps, could be easier; but it was a remarkable fact, that, of all the inhabitants of Windsor Castle, Prince Albert was the only one attacked. Again, near Edinburgh, we have meadows irrigated with sewage, from which foul smells and noxious exhalations have been escaping for 200 years; and yet, notwithstanding the magnitude of the nuisance, enteric fever in that locality had never occurred. Again, the night-soil men of Paris, and those who perambulated the drains of London and Paris, were amongst the healthiest of the race. Moreover, to escape from smells and water-closets, whither can we go?—these must exist where excrement exists. But he conceived that all presumption was against considering them as the essential cause of fever.

*Mr Benjamin Bell* stated that, at all events, occasionally something more than mere emanations was at work. Some years ago, in two houses connected together in Edinburgh, there were sixteen cases of fever, with a tendency to bleed per anum; two of these died. He himself had been interested in one of these cases, and had been associated with a small committee engaged in investigating the cause of the outbreak. There was no bad air, overcrowding, or bad smells; but it was found that in the back-green of one of these houses there was a well highly prized by the inmates for its cool sparkling qualities, and into this one of the drains leaked *guttatim.* This was put to rights, and no further cases occurred.

*Dr Murray* said that, from considerable experience and inquiries, he had no hesitation in stating that enteric fever spread through houses in a common stair by means of the defective ventilation, the exciting cause being water-closet emanations, and that when, by complaint or otherwise, these were made to ventilate outside, or, still better, were altogether removed outside, the result was a great improvement in the health of the inhabitants.

*Dr George W. Balfour* remarked, that Dr Bennett would find considerable difficulty in proving the existence of an epidemic of enteric fever unconnected with poisonous exhalations. He believed that one important reason why Edinburgh was formerly so free of enteric fever in comparison with the present time, notwithstanding the presence of water-closets and of fæcal emanations, was, that in those days the emanations from them were not so concentrated; for he agreed with Dr Murray, that dilution with atmospheric air was a very important agency in neutralizing the poison of such gases. He considered that the great perfection to which our system of drainage was now carried was the great source of danger; and that a system of closed pipes trapped at one end for many hours a day by many tons of sea-water, as we had in Edinburgh, and at the other but by a few inches of fluid, was the very best means of poisoning the atmosphere of our houses. He further believed, that enteric fever and other allied miasmatic disorders had very much increased since our drainage had been so perfected. He had never any difficulty in tracing serious gaseous leakage in any house so affected. Some of his experiences in this matter were very extraordinary, and in every case a proper rectification of this matter, by means of so-called extraction pipes and other necessary means, had resulted in a greatly improved sanitary condition of the house affected. While he believed that gaseous emanations were much the most common source of such affections, he quite agreed with Dr Bell that, in many cases, they were traceable to the solution of the poison in the water drunk.

*Dr Matthews Duncan* said that he had listened with the greatest pleasure to Dr Bennett's very able address, though he considered it erroneous in regard to the explanation advanced of the etiology of the diseases referred to. Bad smells and fæcal accumulations were *per se* probably not the real sources of fever poison, but a careful study of the area involved was sometimes sufficient to prove that defective and imperfect drainage was at the bottom of the mischief. He remarked, that the spread of cholera was often traceable to the throwing out of the stools into back-gardens and middens adjoining houses, their gaseous matters poisoning the atmosphere, and their fluids percolating into the wells, and so reaching the neighbouring inhabitants. The remedies for such a state of matters were proper sanitary regulations as to the disposal of the stools, and the bringing of water for household use in pipes from a source distant from any contamination.

*Dr Grainger Stewart* had been much interested in Mr Bell's statement, affording as it did an additional evidence that the typhoid, like many other fever poisons, may be introduced into the system with the drinking water. It had been found in Clifton, upon one occasion, that a severe outbreak of typhoid occurred in certain houses in a particular street, and it was ascertained that the houses which drew their supply of water from a certain well—at the time contaminated with sewage—those who drank from one particular well were all affected with fever, while those which did not escaped. One of the best proofs of the origin of typhoid fever from fæcal sources which he had himself witnessed, was in an endemic outbreak in Hay's Court, Fountainbridge. There had been great fæcal accumulation, consequent upon defective drainage. Upon clearing away this accumulation, and getting the drains properly looked after, no more cases occurred. With regard to the question of the drainage of towns, which had so close a relation to the subject under discussion, he expressed the opinion that the system at present generally in use, and lately extended in Edinburgh at so great cost, is most unsatisfactory. He advocated the plan, brought forward by Mr Menzies of Windsor, of having separate drains for the rainfall, on the one hand, and the sewage material with water used for domestic purposes, on the other. The rainfall should be carried to the nearest stream; the sewage, which would constitute a tolerably definite quantity of material, should be conveyed to farms near the town, to be used for agricultural purposes. If the rainfall be allowed to mix with the sewage, it renders it too dilute to be of much value, and the quantity in such a climate as ours varies so much, that no arrangement for its distribution over the fields could be made. The fixed quantity of comparatively undiluted sewage could, however, be easily managed. He believed that the results of this plan, in places in which it had been tried, were eminently satisfactory.

*Dr Bennett* considered that it was a great mistake to confound the inhalation of a poisonous gas with swallowing impure water. It was the notion that the effluvium from drains and decomposing organic matter was the specific cause of enteric fever, that he opposed. The water mentioned by Mr Bell as sparkling and delightful was unquestionably contaminated by nitrates and nitrites, as had been well shown by Dr Macadam, and these produced an irritation of the bowels often terminating in enteric fever. Since he had been Clinical Professor, he had only seen one case of enteric fever apparently originating from foul emanations; this was the case of a seaman on board of a Leith and Hamburg steamer carrying herrings, which had got loose and become putrid; subsequent inquiries had, however, revealed the fact, that while in Hamburg he had drunk nauseous water from a pump. He regarded imperfect diet and impure drink as the most common sources of such fevers.

*Dr George W. Balfour* said, that several years ago a severe epidemic of enteric fever had raged for three years in a village with which he was well acquainted. In this village the inhabitants lived very much alike, and all drank the same water, yet the outbreak was confined to the lower half of one side of the village; and the reason for this was believed to be as follows:—Half-way up the village, on the right hand, there was a foul privy and yard in which pigs were kept. The mingled drainage from these dung-heaps, only partially cleaned once a year, ran down the gutter on one side of the village. The first cases of enteric fever occurred in the neighbourhood of the privy, and their poisonous stools were added to the already poisonous drainage, and thus acquiring virulence as it rolled, house by house was gradually affected all down the one side of the lower half of the village, the other inhabitants remaining free.

*Dr Gillespie*, in reply, stated that for twenty years past the inhabitants of Donaldson's Hospital had used similar food and drunk the same water as now, but this had been the solitary outbreak of enteric fever, and he considered it clearly traceable to the escape of foul air through the lavatories which had gradually permeated the dormitories, owing to the defective supply of water to the cisterns in the upper storey for some months, and the consequent drying-up of the trapping. The water-closets were not defective, because water had been regularly carried to supply them. It was some time before the lavatories were suspected,—when they became so, and were rectified, the outbreak ceased. In regard to the Craigentinny and other artificial meadows, he thought the case quite different, for the gases were so diffused through the atmosphere that they became comparatively innocuous. It was possible that the sulphuretted hydrogen prevalent in Naples acted as a general disinfectant, supposing Dr Bennett's statement as to the absence of fever from that city, in spite of its insanitary condition, to be true.

## OBSTETRICAL SOCIETY OF EDINBURGH.

### SESSION XXIX.—MEETING V.

*28th January* 1870.—Dr BELL, *President*, in the Chair.

### I. TWO CASES OF PLACENTA PRÆVIA. BY DR ANGUS MACDONALD.

CASE I.—On Tuesday forenoon, 27th July 1869, I was called to see Mrs ——, G—— Street, who had been seized that morning about 8 o'clock with a severe flooding while she lay quietly in bed. She stated, that on perceiving the blood come rushing from her, she started to her feet in alarm, and that the discharge produced a large pool of blood upon the floor. She was pregnant, and, according to the best of her judgment, about the end of the seventh or the beginning of the eighth month of the period of utero-gestation, and expected her delivery about the end of September or the beginning of October. It was her seventh pregnancy, and her labours had all been natural, nor had she ever previously suffered from any of the accidental complications of pregnancy. Up to this date, during her present pregnancy, she had observed nothing abnormal except a feeling of unusual fulness throughout the iliac and hypogastric regions. At this period she was in the possession of excellent health.

On examination per vaginam, I found the cervix soft, the os somewhat patulous and distensible, but not sufficiently dilated to admit my forefinger. There was no longer any hæmorrhage. The presentation was manifestly a vertex. But the head appeared to be high up, and the cervix, when felt, seemed separated from it by a soft, cushion-like mass, which occupied the upper portion of the pelvis. The fœtal heart was distinctly audible on the left side, and was of good strength, and regular. The mother's pulse was good, beating at about 72 per minute, and showing no signs of being affected in character or strength by the loss of blood. I at once suspected I had to deal with a case of placenta prævia; but as the state of the os forbade the possibility of my making the diagnosis complete, I ordered my patient to remain quiet, either on a sofa or in bed, and to take at every six hours' end a tablespoonful of a mixture containing fifteen minims of diluted sulphuric acid. As yet no pains had been felt. There was no recurrence of the hæmorrhage till Friday (30th) forenoon, when it again came on, but this time in much less amount. On Sabbath (1st August) I was summoned out of church during the forenoon service to see my patient, who had experienced a very severe recurrence of the hæmorrhage about half-past 10 o'clock A.M. I arrived about a quarter to 12 o'clock noon, when I found my patient in bed, with the hæmorrhage still going on rather profusely at intervals. There were now slight pains felt from time to time in the abdomen. The cervix was dilated to about the size of half-a-crown. The placenta was felt as a soft, spongy mass, covering the whole os, and attached all round the cervix. I sent for some solution of the perchloride of iron in glycerine and a fresh sponge. A piece of the sponge of suitable size I first steeped in the iron solution, and then introduced wholly inside the cervix through the os. This had the effect of stopping the hæmorrhage almost entirely all that afternoon and evening. The patient meantime continued to take the astringent mixture, and the room was kept strictly cool, while a cool regimen was enforced, and she was made to remain constantly in the supine position. About 11 o'clock P.M., the hæmorrhage beginning to be again rather free, I called on Dr Cappie to inform him of the case, and to ask his assistance in case I required to interfere actively during the night, owing to any sudden increase of the bleeding. Dr Cappie kindly promised to be at my call if I should need him. During the whole of Sunday night and the following Monday the hæmorrhage went on slowly on the whole, but with occasional exacerbations. The os was gradually dilating, and the sponge which I had introduced on the Sunday was expelled along with some clots on the Monday afternoon. By occasional auscultation, I ascertained that the child's pulsations were gradually becoming feebler; and as by 9.30 P.M. on Monday the continuous hæmorrhage was beginning evidently to tell on the strength of my patient, and seeing that notwithstanding the shortness and irregularity of the uterine contractions, the os was now so far dilated that I thought the hand could pass through it, I determined that it was now full time to interfere actively, and accordingly sent for Dr Cappie. He arrived at about a quarter past 10 o'clock. He fully agreed with me in thinking that the safety of the mother would not admit of further delay. Seeing also that the partially dilated cervix, greatly obstructed as it was by the large placental mass which occupied it, though it would admit the hand, presented so manifest obstacles to speedy delivery as to render it very problematical whether it would be possible to pull the child alive through it; and seeing, moreover, that it was exceedingly doubtful if the child was as yet viable, while it was certain from auscultation that it was becoming very weak, we resolved to give up all attempts at saving the life of the child, and to separate the whole of the placenta from its uterine attachments, with the view of stopping the hæmorrhage, and thereby saving the mother from further risk. Dr Cappie accordingly put the patient deeply under the influence of chloroform, after which I introduced my hand with some difficulty through the os, insinuating it between the cervix and the placental mass. At this time the bleeding was very free. I now ascertained that the placenta, though attached round the cervix in all directions, extended more especially backwards and upwards along the posterior wall of the uterus, in which direction its attachment reached almost to the fundus uteri, while anteriorly its attachment stretched from one to two inches in front of the junction of the placenta with the dilated cervix. I carefully peeled it off from the whole of its attachment to the uterus; and whereas the bleeding had previously been very profuse during the whole of the time I was operating, the moment I had completed the entire separation, the bleeding absolutely ceased. This took place before the membranes were ruptured. To make more certain of this fact, I asked Dr Cappie to examine and ascertain the truth of the observation. I then ruptured the membranes, after which the pains became in a short time both regular and powerful, and the labour was safely completed by the unaided uterine efforts at half-past 1 o'clock on Tuesday morning (2d August). The patient made a rapid and good recovery, and was able to leave for the country three weeks after her confinement.

CASE II.—On Saturday evening, 8th January 1870, I was asked by one of the New Town Dispensary pupils to see Mrs Gormly, residing in Scott's Close, Cowgate, who had been suffering from uterine hæmorrhage. The patient's age was 42. The present was her fifteenth pregnancy, but four of these—viz., the third, fifth, seventh, and ninth—had been miscarriages, occurring about the third month. My pupil had been first called to see this case on Tuesday morning, the 28th of December 1869, when he was informed that during the night she had been suddenly seized with profuse hæmorrhage, but had felt nothing like uterine pains. The patient was rather excited, and he failed to get her persuaded to allow him to make a digital examination. He accordingly left, after enjoining her to keep quiet in bed.

On the 29th, on examination per vaginam, he felt the os sufficiently patulous to allow the point of the forefinger to enter it for some distance. He was able also to make out that the presentation was a head, but failed to discover any portion of the placenta. For the previous twenty-four hours, the amount of bleeding had been very trifling. He ordered the following mixture:—℞ Acidi gallici, gr. xx.; acidi sulph. dil., ʒi.; tinct. opii, ʒss.; aquæ, ℥iv. M. Sig., "A tablespoonful every three or four hours, when the bleeding appears." From this date up to the afternoon of the 7th of January 1870, only slight bleedings occurred at intervals of about two days, but no uterine pains were perceived. During this period the patient had repeatedly remained out of bed during the greater part of the day; and on one occasion had walked to the Grassmarket and back, a distance of about two hundred yards. On the forenoon of Saturday the 8th January, the pupil, on visiting his patient, found that there had been a sudden gush of bleeding the previous afternoon, which, however, ceased so soon as the patient lay down in bed. Beginning to feel rather uneasy about those repeated bleedings, he asked me to see the patient. After first ascertaining that the hæmorrhage was still going on, though not at all profusely, that the placental soufflet was loudest over the lower third of the abdomen, that the fœtal pulsations were strong, about 120 a minute, and that she was, as she thought, at her full time, we put the patient under the influence of chloroform to facilitate the digital examination, as she was in a highly irritable and nervous state of mind. On examination per vaginam, the os was discovered to be dilated to from one and a half to two inches in diameter, and during the exploration a slight contraction was felt which slightly distended the membranes. The os was high up in the pelvis, the cervix thick, but soft and dilatable. About one half of the area of the dilated os towards the left, and backwards, was found to be covered with the edge of the placenta, which was distinctly felt attached to the bag of membranes, and to be separated into two nearly equal portions by a fissure in its substance, passing from before backwards. As the hæmorrhage was not great, as the uterine contractions were almost entirely absent, and as the os uteri was not wholly occupied with the placenta, I deemed it very likely that, when the head came down, it would arrest the hæmorrhage entirely, and that, consequently, there would probably be no occasion for any interference. The patient was enjoined to keep strictly the recumbent position, and having asked the pupil to send for me should either labour-pains fairly set in or hæmorrhage supervene, I left. He waited with the patient till 12 P.M., but as neither labour-pains nor hæmorrhage came on, he also went home. We saw her again at 11 A.M., on Sunday, 9th January, but found that neither pains nor bleeding had recurred during the night. We also called at 9 P.M. same evening, but found that a small clot and a slight trickling of blood had been all the hæmorrhage which had appeared since morning. I recommended that the opium should be kept out of her astringent mixture, in case it should retard the supervention of uterine contractions.

*Monday*, 10*th January*.—Neither bleeding nor labour-pains had occurred up to the time she was visited by the pupil.

*Tuesday*, 11*th*.—My pupil was told, on visiting her about mid-day, that the membranes had ruptured about 7 o'clock in the previous evening. But there had been little or no hæmorrhage, though the uterine contractions had been pretty regular and strong. He then informed me of the state of matters, and I saw the patient about 4 o'clock P.M. The os was then well dilated, but the placenta was being moved forwards to the right with the force of the pains, and evidently rather inclining to come down in front of the head, which still remained high up; so that now fully two-thirds of the area of the dilated os was covered with the placenta, instead of barely one-half of it, as on the former examination. The membranes had ruptured anteriorly near the extremity of the fissure in the presenting portion of the placenta; and at the site of the rupture, as well as backwards, lying immediately behind the fissure in the placenta, the cord could distinctly be felt pulsating. There was occasionally considerable hæmorrhage with the pains, but not with every pain, nor yet to such an extent as to cause the slightest anxiety regarding the patient. I accordingly waited with her till half-past 5 o'clock, when, on examination, I found that there had occurred a very extensive prolapse of the cord; and seeing that the cervix was sufficiently dilated to allow of turning, I felt convinced that version presented the fairest chance of effecting delivery with safety to the child. Therefore, without wasting time in endeavours to replace the cord, which was found still to pulsate strongly, by altering the position of the mother, or otherwise, I had the patient put under the influence of chloroform; and pushing up the cord into the interior of the uterus, as I introduced my hand past the anterior edge of the placenta, I got hold of the right foot of the child (the back of the child was turned towards the right of the mother's spine), and brought down the breech so as to engage in the pelvis without involving any loop of the cord. Finding, by the pulsations of the interior tibial of the child, that its circulation was in no way impeded, and there being no hæmorrhage, the pains were allowed almost entirely unaided to slowly push the breech and lower portion of the body of the fœtus through the passages. After the child was thus far advanced, it became evident that the cord was being somewhere compressed, and the labour was accordingly hastened. The child did not breathe well for some little time after delivery, but ultimately came round and cried lustily. Some clots of internal hæmorrhage came away immediately after removal of the fœtus. The uterus contracted well after delivery, and the placenta separated a few minutes after the child was expelled. There was no post-partum hæmorrhage. During the operation of turning, the pulse rose to 130 per minute from 110 per minute, at which it was beating before we commenced to interfere actively; but by 12 P.M. the same evening it had again fallen to 110. Both mother and child did well.

*Remarks.*—Placenta prævia is admitted on all hands to be one of the most dangerous accidents with which the obstetrician is called to deal, and the importance of the subject will, I hope, be taken as a sufficient excuse for my taking up the time of the Society with these two cases. A complication of parturition which results in the death of from one in three to one in three and a half of the mothers who are unfortunate enough to be the subjects of it, ought, if possible, to be well understood, and at any rate to be made the subject of frequent discussion among accoucheurs, for "in the multitude of counsellors there is wisdom," and nothing so much tends to the elucidation of truth as dispassionate and earnest discussion.

Now, the two cases just recorded seem to me to be wonderfully typical cases—the first of severe and complete, the second of mild and partial placenta prævia. The first case was marked by severe flooding, commencing at least two months before the natural date of parturition, inducing the supervention of labour-pains prematurely; and, in consequence of the serious risk the mother was placed in, necessitating active interference. The second did not supervene till about the end of the full period of utero-gestation, occasioned only comparatively slight hæmorrhages, and, but for the unfortunate prolapse of the cord, seemed likely to terminate, if left to the efforts of nature, unaided, with complete safety both to mother and child. But the former case is chiefly valuable on account of the confirmation which it gives of the value and efficacy of the mode of treatment by complete separation of the placenta, recommended first by Sir James Y. Simpson, and defended with such ingenuity and ability, besides being supported by such an array of stern facts, in his articles on placenta prævia in the first volume of his Obstetrical Works. Obstetricians may dispute as to the origin of the hæmorrhage, and as to whether it is arterial or venous, as long as they please; but I can imagine no one who has candidly read those papers able to doubt that complete removal of the placenta does almost invariably put an end to hæmorrhage in cases of placenta prævia. This case is strongly affirmative of this thesis. While any part of the placenta remained attached to any portion of the uterus, the hæmorrhage went on profusely; but so soon as complete separation was effected, it stopped immediately and absolutely. Now, this result was not due to any pressure of the child's body against the placental surface, for I noticed the arrest of hæmorrhage, and had the observation, for the sake of greater accuracy, confirmed by Dr Cappie, before the membranes were ruptured, so as to render such compression possible. The stoppage of hæmorrhage before the rupture of the membranes seems also to negative the idea that its arrest is the result of contraction of the uterine walls, for no great change of area in their surface could take place so long as the membranes were intact. This case also tends to prove that the position taken up by Dr Tyler Smith, viz., that *removal* of the placenta is not at all so likely to act as a means of arresting hæmorrhage, as its *expulsion* before the child by the uterine efforts, is at least very doubtful. In this case there was at the period when the afterbirth was removed, and, indeed, up to the moment when the membranes were ruptured, little or no uterine action, and yet the removal of placenta effected immediate and complete arrest of the hæmorrhage. Although I used to be decidedly inclined to the opinion that uterine hæmorrhage was in such cases directly from the free uterine surface, and not indirectly first through the portion of the uterus to which the placenta was still adherent, and finally from the free placental surface, I confess that this case has read me a lesson, and made me incline strongly to the less obvious opinion, that the hæmorrhage is indirect. It is often urged that the irritation of the hand during the act of removal of the placenta so irritates the uterine fibres in the placental site that they occlude the vessels and prevent further regurgitation of blood. But my difficulty in acceding to this suggestion is this, If it be true, why does it require the whole placenta to be separated? Ought not the irritation of removing a portion of the placenta, or at least so much of it as can be affected by the change in circumference of the lower portion of the uterus during a pain, to be quite sufficient? If irritation is an effective hæmostatic, and if the free surface of the placenta is not itself the chief source of hæmorrhage, a partial separation ought for a time at least to be equally effective with complete separation. But this is found not to be the case.

It may be argued by those who are more inclined to the method of delivery by version in such cases, and who have rather faltering faith in the efficacy of complete removal, that we did wrong in removing the afterbirth, and that we ought to have attempted to turn. Well, we carefully weighed this consideration, and both Dr Cappie and myself were of opinion that there was next to no chance of effecting delivery by version so as to save the life of the child—the os was so imperfectly dilated, and the placental mass so obstructed the brim of the pelvis; besides, there was grave reason to doubt whether the child was as yet viable; and, moreover, from careful auscultation, I had already ascertained that the child's heart, from being some days previously strong and audible, had by this time become exceedingly weak and almost inaudible. I hold that it ought always to be with the accoucheur a matter of the most serious thought, and ought to demand his most faithful and conscientious consideration, whether in undertaking any operation involving risk to the life of, or certain death to, the child, he is clearly guided by sound principles of action. Now, we fairly considered this position, and were agreed in thinking that the extremely problematical chance of saving the child's life, even if it were viable, was not to be entertained, since by avoiding turning we lessened undoubtedly the risk to the mother. The extremely happy issue to the latter I think fully justified our action. It is not fair to say, that if there was room to introduce the hand and remove the placenta, there must have been room to turn; for it is not the mere effect of turning that is dangerous to the mother, but the forcible distention of the preternaturally lacerable cervix uteri.

Without, therefore, attempting to decide in regard to the venous or arterial origin of the bleeding, I think this case tends to prove that the bleeding in placenta prævia is with greater probability referrible to the torn placental surface than to the part of the uterus laid bare by its removal; while it shows, moreover, that be this true or not, *practically* removal of the whole placenta is a powerful means of arresting hæmorrhage in such cases.

In reference to the treatment of the second case I have little to remark, except that, considering the large amount of the placenta which was separated, it is astonishing that more hæmorrhage did not take place; and that, had it not been for the evident tendency of the placenta to come down in front of the head so as to threaten to be expelled before the child, and also had not the prolapse of the cord taken place, the case might safely have been allowed to terminate itself. But seeing there was this double danger to the life of the child, I think I was fully justified in turning so as to save its life.

The usual doctrine of the causation of placenta prævia receives confirmation from these two cases. Both happened in women who had been repeatedly pregnant, and consequently it might be reasonably expected that in both cases the cavity of the uterus was large, and thus allowed the ovum to roll down to the os before it became embedded in the decidua vera.

*Dr Keiller* said it was quite true that, even when there are no uterine contractions, separation of the placenta is not followed by any bleeding. He remembered attending a case in which there had been repeated floodings, and on passing the hand up, with a view to ascertain where the placenta was situated, such gushes of blood came on, that he separated the afterbirth, and turned the child. And in this case the child was born without any uterine pain, and the womb contracted perfectly. In such cases we ought not to be afraid of early interference; indeed, the mistake was often made of delaying too long. He remembered being called to a twin case, and on his arrival one child was already born, and the double placenta was lying between the mother's legs. The second child was coming down by spontaneous expulsion.

*Dr Pattison* said he had met with a case in which separation of the placenta checked hæmorrhage.

*Sir James Simpson* remarked that Dr Macdonald had not alluded to one kind of practice, namely, partial separation of the placenta. He remembered long ago being told by Dr Tait, that when in practice at Lauder, he had been hastily summoned to a patient, and when he arrived, he found the woman walking about with the placenta dangling between her legs, and the hæmorrhage had stopped; and this was what led Sir James first to think of separation of the placenta in cases of placenta prævia. The first time he introduced the practice was in the case of a patient of Dr Hill's at Portobello. The placenta was partially through the os, and having pulled it out, the bleeding ceased. It was an error, perhaps, to separate the placenta and afterwards turn, except to save the child. The late George Wilson used to fancy, that if the placenta were put into oxygenated water, the child might thus be kept alive. Sometimes the hæmorrhage might be stopped by simply separating a little more of the placenta than what had already been detached; for, as the bleeding came from the line of vessels between the placenta and the uterus which are on the stretch, the separation relieved the tension, and by this practice there is a greater chance of saving the child.

*Dr Cairns* was of opinion that Dr Macdonald should have treated the first case exactly in the same way as the second. The first reason given for not doing so, as to the viability of the child, he did not think a good one; and the second reason, namely, that the os was not sufficiently dilated, was not any more satisfactory. It had been stated that the os was about the size of a crown-piece; and if that was so, he was surprised that Dr Macdonald should have allowed the patient to go on bleeding for two days.

*Sir James Simpson*, in reply to Dr Cairns, said that, no doubt, one reason why Dr Macdonald did not turn was, the difficulty in dilating the os, and the danger of tearing the cervix.

*Dr Macdonald* replied to Dr Cairns's observations.

### II. NEW FORM OF GALVANIC PESSARY.

*Dr James Young* made the following remarks on a new form of galvanic pessary, which he had been in the habit of using for some time. He said that, with Sir James Simpson's galvanic pessary, he had experienced in many cases the greatest difficulty in introducing it into the cervix uteri, especially in unmarried patients, in consequence of the large bulbous head. He had caused Gardner to make several galvanic pessaries, of the usual length and thickness of stem, but with a small button-sized head or bulb, so that the difficulty he and others had experienced in their introduction might be removed. Dr Young said that he had found the use of galvanic pessaries attended with the greatest benefit in the treatment of amenorrhœa, where the usual remedies had failed. While he had experienced difficulty in their use, where the bulb was too large, there must be great care exercised in not having it too small—as in one case of his the pessary slipped up into the cavity of the uterus, involving considerable difficulty in its removal. He recommended that the head of the pessary should be in diameter five-eighths of an inch, and in thickness about one-eighth of an inch.

*Dr Keiller* said that he had seen the case alluded to, and, upon examination, he felt the twist of the pessary through the back part of the cervix, and on dilating, he felt the point of the stem. After chloroform was administered, he pressed the bulb of the instrument downwards through the cervix.

# Part Fourth.

## PERISCOPE.

### SURGERY.

RECTO-VESICAL LITHOTOMY.—Dr Louis Bauer reports the following interesting case at considerable length. We can give merely an abstract.

Mr T., æt. 26, was found to have a stone in his bladder. In July 1859, Dr Bauer, assisted by Dr Marion Sims and Dr Thomas Emmet, performed the following operation:—

The rectum was kept open by Sims's larger speculum. The contours of the prostate gland were well exposed; and by the guide of the left index finger, I transfixed the recto-vesical septum immediately behind and at the middle of the base of that organ. This part of the operation was performed by a double-edged scalpel. The blade being about half an inch in width, the wound of necessity could not have been larger. The loss of blood was trifling, and no pain was experienced by the patient, who had not taken chloroform. In fact, he stated that the first intimation he had received of the progress of the operation had been from the finger in the bladder. The extraction of the calculus was effected by a common polypus forceps without impediment. The elasticity of the recto-vesical septum obviously exceeded the requirements of the case.

Dr Sims acceded to my request in closing the wound with silver wire sutures, and so effectually did he accomplish the task that not a drop of urine escaped during the entire after-treatment.

The operation occupied but a short time, and the patient suffered only from the fatiguing position. An elastic catheter was put in the bladder and opium given.

In the next six days his urine was drawn off by catheter. On the seventh he made it *per vias naturales*. On the eighth all the stitches were removed, and the wound was found to be quite healed; and next day he was able to drive twenty miles. The calculus when dried weighed 720 grains.

Dr Bauer also reports the case of a calculus weighing 646 grains, which he removed from the bladder of a female by the vesico-vaginal operation with similar success.—*St Louis Medical and Surgical Journal*, No. 1, 1870.

STATISTICS OF EXCISION OF KNEE-JOINT.—Dr Lucien Pénières has collected from English, German, and American journals nearly 600 cases of resection. Of these 421 were performed for white swelling. The analysis of these 600 cases, along with those observed by the author in the Parisian hospitals, has led Dr Pénières to conclusions which he reduced to the following formulæ in a work recently published on the subject of resection of the knee.

The mortality after this operation is 30·7 per cent., and is less than the mortality after amputation of the thigh for white swelling.

Between the ages of 5 and 35, resection should always be preferred to amputation in uncomplicated cases, if the disease is limited to the heads of the bones, is not of too long standing, and has resisted all other treatment.

Considerable shortening, due to the arrest of the development of the limb in the child, is not an inevitable consequence. It is in many cases the result of the lesion which rendered the operation necessary rather than of the operation itself. It does not follow the operation, unless the lower end of the femur has been entirely removed with the epiphyseal cartilage. It should not be a contra-indication of the operation.

The patella should be removed. It diminishes the length of treatment, and the risks of death and of secondary amputation.

The debris of the capsule ought to be removed with great care, as well as any *fungosities* of the synovial membrane.

Extension of the limb should be performed on the operating table, while the patient is still under the influence of chloroform, and immobility should be secured by appropriate means during the whole of the future treatment of the case.

A tonic and strengthening regimen should be employed during the whole after-treatment; the dressing should be simple, and should not interfere with the splints.

The mortality in resections for anchylosis is less than in operations for suppuration of the joint; and in consequence of this less risk, the operation is warrantable in cases where the angularity interferes with walking, and where the patient is losing strength from pain.

Resection for gunshot wounds of the joint has been followed by good results in civil practice; it should be preferred to amputation in uncomplicated cases, limited to the heads of the bones.

In military surgery, more experience is needed before it can be definitely settled as to the value of resections in gunshot wounds.

Resection has been employed several times with success in penetrating wounds of the knee-joint of great extent.

Amputation should always be preferred to resection in cases of malignant tumour of the knee.

Partial resections have invariably given inferior results when compared with complete ones, and ought never to be performed. An exception may be made in favour of excision of the patella alone, which has been excised with good result in certain chronic affections. —*Gazette des Hôpitaux*, 1869, No. 123.

DEATH BY THE GUILLOTINE has usually and rightly been regarded as one of the most speedy and painless modes of exit from this world. Quite recently, however, about the time of Troppmann's execution for a series of horrible sensational murders, which had excited the nervous system of the world to an almost unbearable degree, a medical man named Pinel took upon him to cap these horrors by making the startling announcement that death by the guillotine was one of the most horrible of deaths, because, by reason of the suddenness of the process, life was retained in the head for at least three hours after death. This statement, garnished by equally logical and rational physiological explanations, and accompanied by a host of sensational stories, went the round of the newspapers and startled the public mind not a little. In a recent communication to the Société de Médecine Légale, M. le Docteur Evrard (de Beauvais) and M. le Docteur Beaumetz have related a series of experiments which they had made upon a head which was delivered to them five minutes after decapitation. The head was placed upon a table, covered by compresses to absorb the blood which might flow from it. The face was of one dull, uniform, waxy hue, the lower jaw fallen, and the mouth open. The expression was that of stupor, not of suffering. The eyes were open, fixed, looking straight before them; the pupils dilated; the cornea had already commenced to lose its lustre and transparency. A few particles of sawdust stuck here and there upon the face, but none were to be found inside the lips or on the tongue. The external meatus was carefully cleansed, and the experimenters, bringing their lips as close as possible to the ear, thrice called out the criminal's name with a loud voice. No movement of the features or eyes resulted. A piece of charpie saturated with caustic ammonia was next placed under the nostrils; neither the alæ nasi nor any of the facial muscles moved. The conjunctiva of each eye was deeply and successively cauterized with nitrate of silver; the flame of a candle was brought within the distance of two centimètres from the cornea, yet no contraction was observed in the eyelids, eyeball, or pupils. The organs of sense absolutely refused to give any response, either by their functional or more purely physical sensibility. Various incisions were then made through sundry nerves and muscles. The muscles alone gave signs of life; they contracted energetically under the knife, but no grimace, no reflex action, no sign of nerve life, was observed. And the muscular contractions were produced by galvanism even so long as half an hour after the brain was extracted, and when the skin was already cold, but no one could suppose that psychical life then existed; and we think our authors perfectly justified in the conclusions they have arrived at, which are, moreover, identical with those attained after a similar series of experiments by the Medical Association of Mayence in 1803,—viz., that "the sensation of external impression does not persist for one instant after decapitation;" and there can be no doubt of its truth. Without following our authors into their researches into the state of the thoracic organs, which present nothing bearing upon this point at all, and nothing, at any rate, either novel or important, we would only wish to record further that they found no fluid in the arachnoid cavity; that the vessels of the pia mater were almost bloodless, and filled with aeriform fluid; the lateral cavernous sinuses were absolutely bloodless; the ventricles contained scarcely a teaspoonful of fluid; and in no part was the brain injected.

The falling of the lower jaw, and its subsequent closure by cadaveric rigidity, is, in our authors' opinion, quite sufficient to explain the horrible stories of heads biting one another, as related by Sanson and other executioners, so far as these stories are not mere lies. Heindreich, the present executioner, has never observed any other sign of life than two or three slight movements of the lower jaw, similar to those so often seen at the last in those dying from other causes.

Those who are acquainted with "Les Mille et un Fantomes" of M. Dumas will be at no loss to account for the sensational stories of M. Pinel, and will readily trace the source of his physiological reasoning. But Dumas goes a little beyond Pinel, because he makes a head, two hours after decapitation, not only call out the name of its lover, but actually recognise him by dim candlelight through a "sac," while the eyes dropt tears of sorrow! Our authors, in a historical introduction, establish the rights of Dr Guillotin to the resurrection of the fatal instrument named after him, but claim our Scottish Maiden as its earliest prototype; this, they say, was in use in a "Scottish clan" in the sixteenth century.—*Gazette Hebdomadaire*, 18th Feb.

# Part Fifth.

## MEDICAL NEWS.

Relapsing Fever has broken out in New York. We hope careful inquiries will be made as to its origin there. It still spreads in Edinburgh, and its *foci* are becoming more numerous.

British Medical Association (Annual Meeting, 1870).—In reminding our readers that the Annual Meeting of the British Medical Association will be held in Newcastle-upon-Tyne, in August, commencing on Tuesday the 9th, we wish more particularly to direct their attention to the Annual Museum, which the Local Committee are most anxious should be well furnished with new objects of interest and instruction.

To this end, we ask for their kind co-operation, and beg to express our hope that they will be able to contribute one or more specimens, such as:—

I.—New instruments and appliances in medicine, surgery, and midwifery.
II.—New drugs and preparations.
III.—New books, English and foreign.
IV.—Pathological, physiological, anatomical, and microscopical specimens.
V.—Photographs, drawings, casts, and models of pathological specimens.
VI.—Models of new inventions, relating to public health, etc.
VII.—New preparations of food, etc.

Rooms will be provided at the Newcastle Infirmary, for the Museum, which will be opened on Tuesday morning, 9th August, and closed on Friday evening, 12th August.

All the objects intended for exhibition must be addressed, "Care of House-Surgeon, Infirmary, Newcastle-upon-Tyne," be delivered on or before Monday, 2d August, and be removed on or before Monday, 15th August.

Every object must be accompanied by a written or printed description, together with a short reference, for insertion in the catalogue.

Adequate space and the necessary fittings for properly exhibiting the objects will be provided, but all expenses connected with packing and carriage, and all risk from injury or loss, must be borne by the exhibitors.

Intending exhibitors are requested to apply to Dr Banning, Gateshead-upon-Tyne, for any information that they may require, and to inform him, as early as convenient, what they intend to exhibit, and how much space they are likely to require.

Exhibitors who may prefer personally delivering their specimens, are earnestly requested to forward a short description, on or before Monday, 2d August, in order that the catalogue may be complete.

## GENERAL COUNCIL OF MEDICAL EDUCATION AND REGISTRATION. SESSION 1870.

ROYAL COLLEGE OF PHYSICIANS, LONDON,
*Thursday, 24th February.*

THE General Medical Council met on Thursday the 24th February, at the College of Physicians, London, at 2 P.M., for the despatch of business. The following members were present:—Dr Paget, *President*, in the chair; Dr Bennett, Mr Hawkins, Mr Cooper, Dr Acland, Dr Humphry, Dr Embleton, Dr Storrar, Dr Alexander Wood, Dr Andrew Wood, Dr Fleming, Dr Macrobin, Dr Thomson, Dr A. Smith, Mr Hargrave, Dr Leet, Dr Apjohn, Sir D. Corrigan, Bart., Dr Sharpey, Dr Parkes, Dr Quain, Dr Rumsey, Dr Stokes. Dr Francis Hawkins, *Registrar*.

Dr George Murray Humphry was introduced, and took his seat in the Council as the representative of the University of Cambridge.

The President then opened the proceedings in a short speech, explanatory of the reason for their having been summoned at this early period of the year, and expressing regret at the absence of Dr Christison.

The usual Commitees were then appointed, and thereafter the following communication was read from the Lord President of the Privy Council:—

Medical Department of the Privy Council Office, 2d Feb. 1870.

SIR,—I am directed by the Lord President of Her Majesty's Council to state to you that, since his recent interview with the Executive Committee of the General Medical Council, and with the assistance of such information as you were then good enough to lay before him, he has given further and most careful consideration to the question of an amendment of the laws relating to the medical profession.

The Lord President, as you are aware from the letter which, by his Lordship's direction, I had the honour of addressing to your official predecessor, Dr Burrows, on the 14th of May last, would have the greatest pleasure in finding himself able to co-operate with your Council in any course tending to promote the development of the medical profession, but could not, on the part of Her Majesty's Government, bring before Parliament any Bill purporting to be for amendment of the Medical Acts, unless he believed it substantially to cover all the ground where amendment of those Acts is called for, and likely in consequence to be for some time a settlement in that branch of legislation.

Now, the Lord President's further consideration of the system under which candidates are at present admitted to legal qualification for practice, has greatly increased the misgivings with which his Lordship expressed himself on that subject in his former communication to your Council; and the Lord President thinks it certain that no new legislation could have in it a fair prospect of permanence, or could, even for the time, be satisfactory to the profession and the public, unless it effected, or promised to effect, some very considerable improvement in that system.

That Examinations for admission to the Medical Register are held by many mutually independent, and in great part competing, corporations, and that each Examining Board has its own separate set of conditions for admitting candidates to examination, is the system which now exists under supervision by your General Council; and, whereas, the Lord President's former communication adverted to some of the evils which are prevailing under that system, other evils incidental to it, and the obstacles which in the present state of the law delay such evils from being remedied, are abundantly shown in papers which you have communicated to his Lordship, and in other representations which have reached him.

The Lord President doubts whether the present system, under any practicable kind of supervision, can either work satisfactorily for Medical Education, or can provide adequate and uniform security for those great public interests which are concerned in the efficiency of the medical profession.

His Lordship is persuaded that doubts like these could not fail to be pressed in any parliamentary discussion of the Medical Acts: doubts, particularly, whether the conditions under which candidates are admitted to examination, with a view to minimum-qualification for practice, ought not to be expressed in a single code of regulations common to all Examining Boards of the United Kingdom; and whether, similarly, a more or less consolidated examining authority ought not to be substituted for the many separate Examining Boards which now act under your General Council in giving admission to the Medical Register.

The Lord President believes that opinions adverse to the present system are very generally entertained in the body of the medical profession by persons who may be regarded as competent and impartial observers; and that some of the most important public bodies concerned with the constitution of the profession have recorded resolutions in a like sense, and are even endeavouring to amend the working of the system by voluntary arrangements with one another.

The Report, moreover, which you brought under the Lord President's notice, as recently made to your Council by a Committee specially appointed to consider the subject of Medical Education, expresses itself in terms so unqualified as to an urgent necessity for consolidation of examining authorities, and as to the general grounds on which that necessity rests, that the Lord President regards this question as presumably ripe for decision.

The recommendation of that Education Committee of your Council is, that, "leaving to the Universities and Corporations full liberty to deal as they please with their honorary distinctions and degrees, the Medical Council should endeavour to effect such combinations of the Licensing Bodies included in Schedule (A) as may form a conjoint Examining Board for each division of the Kingdom, before which every person who desires a license to practise should appear, and by whom he should be examined in all subjects;" and your Executive Committee (referring to the possibility that such consolidations might not be adopted on mere recommendation) represents to the Lord President that, "in the opinion of many members of the Council, the Medical Act is deficient in not granting compulsory power to effect such amalgamations of Examining Boards as may seem desirable."

Accordingly, in view of all the circumstances, the Lord President would not be willing to submit to Parliament any proposals for amending the Medical Act in such minor respects as were touched by the Council's Draft Bill of last session, unless he were, at the same time, in a position to propose larger improvements in the present system. But if it be the wish of your Council that legislation in this larger sense should be asked for, particularly such legislation as your Executive Committee seems to have in view, the Lord President would hope to be able to propose to Parliament a Bill to give effect to such intentions.

The Lord President therefore directs me to request that you will have the goodness to take the sense of the Council on this question. And I am also to state to you that his Lordship would have little hope of carrying any measure in this next session of Parliament, unless he were enabled to introduce it without any considerable delay.—I am, Sir, your obedient servant,

To the President of the General Council, etc. (Signed) JOHN SIMON.

On the motion of Dr Bennett, seconded by Dr Storrar, it was agreed that the consideration of this letter should take precedence of all other business on the morrow. And on the motion of Dr Alexander Wood, seconded by Dr Andrew Wood, it was agreed that the hour of meeting should be altered to one o'clock.

The following letter from the King and Queen's College of Physicians in Ireland was then read:—

King and Queen's College of Physicians in Ireland,
22d February 1870.

SIR,—I am directed by the President and Fellows of the King and Queen's College of Physicians in Ireland to inform you that, having maturely considered the subject of the introduction of an amended Medical Bill, they are of opinion that any Bill introduced without the previous step of the issuing of a Royal Commission to inquire into the working of the several Medical Acts, especially in regard to the last eleven years, during which the General Medical Council has been in action, would almost certainly prove unsatisfactory, and would probably be opposed.

From their knowledge of past occurrences, the President and Fellows with great reason apprehend that, without such preliminary inquiry and report, there will arise such influences of conflicting interests, contradictory views, and embarrassments of judgment in the Houses of Parliament as will mar the progress of any measure, however useful.

The General Medical Council has now been in action for more than eleven years; and it may be fairly expected, from the long experience and knowledge of its members, aided by the information of members of the profession generally, that such a mass of information will be collected as, when embodied and impartially analyzed in a Report, will furnish a satisfactory basis for the introduction of a good Bill, and will carry with it an amount of trustworthy information and authority that will insure a general assent to its provisions.

The President and Fellows think that it is only by the adoption of this course a satisfactory result will be attained, and they therefore earnestly solicit the attention of the General Medical Council to their recommendation.—I have the honour to remain, your obedient servant,

(Signed) JAMES LITTLE, *Registrar*.

Francis Hawkins, Esq., M.D., Registrar, General Medical Council.

On the motion of Dr A. Smith, seconded by Dr Rumsey, this letter was agreed to be entered on the Minutes.

A communication from the President and other Officers of the Medical Reform Union was next read:—

### AMENDMENT OF THE MEDICAL ACTS.

7 Waterloo Street, Birmingham, 21st February 1870.

MR PRESIDENT AND GENTLEMEN,—When we had the honour of being received by you as a deputation, in July last, to present a memorial from the members of the medical profession, concerning the amendment of the Medical Acts, 5200 registered practitioners had signed that document. Four thousand five hundred and twenty-four signatures have since been added to it, so that, at present, the number of assents is 9724; a considerably larger number, we have reason to believe, than has ever been appended to a similar document in the history of the medical profession.

The events of the last few months have proved that our Memorial represents the feelings and wishes of the medical profession throughout the United Kingdom in a still greater degree than is implied by the number and character of the signatures.

At the close of your late session we were favoured, through your Registrar, with copies of the Report of your Committee appointed to consider the amendment of the Medical Acts, and we had intended submitting to you some observations on that Report on the approach of your ordinary session.

It having come to our knowledge that a special meeting of your Council has been convened to consider matters intimately bearing on the proposed amendment of the Medical Acts; and it being evident from your Committee's Report, already referred to, and from the Resolutions of your Council (12th July 1869), founded on that Report, that the prayer of your 9724 memorialists, whom we are deputed to represent, has not yet produced the desired effect upon you, we are anxious to rectify any existing misapprehension, which may be possibly due to the brevity of the explanations offered on your invitation at the personal interview of 7th July last.

We take this opportunity of tendering our sincere acknowledgments of the patience and courtesy with which our remarks were listened to by your Council at the interview just referred to. We deemed it inadvisable to add to the length of your sittings by speeches, and we were under the impression that the Memorial was so clearly, though tersely, expressed, that the wishes of your petitioners could not be misunderstood.

Under existing circumstances we respectfully. beg your attention to an explanatory statement.

The present system of granting licenses to practise finds so few defenders, that the prayer of your memorialists for its abolition can scarcely fail to be successful. We deem it necessary, however, to call your attention to the fact, that your petitioners not only pray that an end be put to the present system of examinations for licenses to practise, which distracts students, fails to protect the honour of the medical profession, and is opposed to scientific unity and to the best interests of the public; but your petitioners furthermore pray that the one legal qualification to be hereafter granted to candidates for admission into the medical profession, shall only be granted on proof being given, by a high standard of examination, that the applicant for the license does really possess a competent scientific and practical knowledge of Medicine and Surgery.

We are convinced that any alteration of the present system would fail to satisfy your 9724 memorialists, if it aimed at protecting and consolidating the interests of the Corporations which now grant licenses to practise, in preference to promoting the real interests of science, of the profession, and the public; we hold these interests to be identical, and we submit that they have hitherto suffered very seriously, in consequence of the line of action pursued by several of the licensing bodies.

We beg to quote your Resolutions Nos. 3 and 4 of July 12th ult., on the Report of your Medical Acts Amendment Committee.

"3. That having carefully considered the objects of the Medical Act of 1858, and the constitution of the Council appointed under that Act to carry out its objects, the Council are of opinion that, for the purposes of the existing Act, the present Council is essentially well constituted."

"4. That the Council are of opinion, that if the Legislature should think proper to invest the Council with extensive powers and fresh duties, by which the profession at large would be brought under the direct influence of the Council, then in that case the profession at large should have more direct influence in the appointment of Members of Councils."

You agree with your petitioners in thinking that the existing Medical Acts need amendment; and even if it be conceded that for the purposes of the existing Medical Act, the present Council is essentially well constituted, it does not follow that the present constitution would be adequate to carrying out the objects of an amended Act. It is generally conceded that the present Council is defective in power, and we take your Resolution No. 4 to be an admission of the justice of our prayer, that provision be made in a new Act of Parliament for the representation on the Council of the general body of practitioners of Medicine and Surgery, who are, for the most part, at present deprived of any professional franchise.

At present the Corporations are dominant in the Council. We feel that we are giving expression to the wishes of the memorialists when we urge that an amended Medical Act should give full power to the Medical Council to govern the Corporations as well as the profession. Such a Council could only command the respect of the profession and the country by being representative of the profession and responsible to Government and to public opinion.

We are not unmindful of the good which the Medical Corporations have wrought, and we shall be glad to see an amended Medical Act, so framed as to enable them to devote their resources to the advancement of science and to the lasting interests of the profession. Our present duty is to convey to you, respectfully, but very candidly, the conviction of the memorialists, that in framing an amended Medical Act the first interests to be considered are the interests of the public and of the profession.

Should you desire any further explanation of the object and intention of your memorialists, we shall be happy to furnish it.

We forward a duplicate of this communication, with a copy of the Memorial, requesting that you will do us the honour of forwarding them to the Lord President of Her Majesty's most Honourable Privy Council, on the first occasion of your communicating with his Lordship touching the amendment of the Medical Acts.—We have the honour to be, Mr President and Gentlemen, your very humble servants,

BELL FLETCHER, *President of the Medical Reform Union.*
SAMPSON GAMGEE, *Vice-President.*
ARTHUR OAKES, *Treasurer.*
D. C. LLOYD OWEN, *Secretary.*

To the General Medical Council of the United Kingdom.

On the motion of Dr Embleton, seconded by Dr Quain, this communication was agreed to be entered on the Minutes. Dr Fleming, seconded by Dr A. Smith, moved "That the Registrar be instructed to inform the memorialists that their communication had been received and read to the Council;" but this was negatived and the motion carried.

The following Report of the Committee appointed by the Council to confer with the Licensing Bodies on the Education Report was then read:—

In accordance with the Resolution of Council (Minutes, vol. vii. p. 94), the Report of the Education Committee, which had been presented, but not discussed, at the end of the last meeting of Council, was forwarded to the Licensing Bodies. Their attention was particularly directed to the suggestion made in paragraph 22 of the Report, that conjoint Examining Boards should be formed in each division of the kingdom, before which every person who desired a license to practise should appear, and by which he should be examined on all subjects; any higher degrees he might wish to take being optional, and taken afterwards.

Answers were requested from them on this point in December 1869, the other portions of the Report being left for further and separate consideration.

Up to the present date answers have been received from thirteen of the Licensing Bodies, and are given in the appendices. Owing to the unusually early period at which the Council has been summoned, it is probable that the answers from several Licensing Bodies will not arrive in time to be laid before the Council, and the same cause will make the present Report less complete than it would otherwise have been.

The questions raised in the Report of the Education Committee referred to matters either of Examination or of Teaching.

### *Examination; the Formation of Conjoint Examining Boards.*

All the English bodies have replied to the question of Conjoint Examining Boards, and it is satisfactory to find how much unanimity exists. The carefully-expressed answers of the Universities of Oxford, Cambridge, London, and Durham, and the decided Resolutions of the Royal College of Physicians of

London, and of the Royal College of Surgeons of England, in favour of such Boards, are decisive evidence of the opinion which prevails in England.

The College of Physicians of London at once proceeded to act on its Resolution, by framing a scheme and by inviting a conference with the College of Surgeons, and subsequently with the Society of Apothecaries; and afterwards communications were made to the English Universities. The results of these conferences, and the final arrangement of the details of the plan, are not yet officially made known, but enough has transpired to show that the anticipated difficulties have nearly been surmounted, and that not improbably when the Council meets the matured scheme will be laid before it.

The case is different in Scotland. Only two bodies, the University of St Andrews, and the Faculty of Physicians and Surgeons of Glasgow, have yet officially replied; the other Universities, the Royal College of Physicians of Edinburgh, and the Royal College of Surgeons of Edinburgh, have all had the subject under repeated consideration, but without as yet coming to any conclusion which can now be announced. It would seem as though more difficulties have been experienced than in England, or the desirability of the plan has not been so strongly felt. The Council will, no doubt, hear from the representatives of the Scotch Universities and Corporations the position of the question, and will be able to judge of the weight of the alleged difficulties.

The University of Dublin, the Royal College of Surgeons of Ireland, the King and Queen's College of Physicians, and the Apothecaries' Hall, have replied from Ireland. The Royal College of Surgeons, in expressing an unfavourable opinion of the suggestion of the Committee, have given in one of the latter paragraphs of their Report a very strong argument in its favour. The University of Dublin has admitted the desirability of a single Examining Board, but the plan it proposes seems not to be the most advantageous way in which the principle can be carried out.

The King and Queen's College of Physicians in Ireland express their want of concurrence in the principle of conjoint Examining Boards, and decline "at present to enter into details as to the mode of carrying out that proposal."

The Apothecaries' Hall of Ireland have, without reserve, expressed their concurrence in such a plan, and have suggested the corporations which should unite.

The Queen's University has as yet made no reply.

To sum up the results so far: In England, the desirability of altering the present cumbrous and inefficient system by establishing a single efficient Examining Board, has been acknowledged and acted on; in Ireland, it has been acknowledged by two bodies, and virtually so by a third; while in Scotland it has not been acknowledged in any case.

Without anticipating the decision of the Council, the Committee believe that the time has now come when the Council should set this question at rest by expressing a decided opinion upon it.

### *Teaching.*

In the absence, at present, of sufficient detailed criticism on the educational matters discussed in the Report of the Committee of last year, it is impossible to recommend the Council to take complete action in this matter. But there are four points on which there is sufficient unanimity of opinion, and on which, therefore, the Council might at once proceed to legislate. If this be done, a very substantial improvement in medical education would be made.

1. The Committee on Education recommended that written class examinations should be enforced in all cases, and that the certificate of attendance on lectures should bear evidence to this. The Society of Apothecaries of London has already acted upon this, and has ordered that after June 1870 this regulation shall be enforced on all who seek its license. As this order will affect all the English schools, and as the system of written class examinations is already in use in several of the Scotch and Irish schools, it is evidently desirable that the Council should give fuller scope to the recommendations of its Committee, and recommend that, from and after the commencement of the

Winter Session of 1870, this custom shall be put in force in all schools, and shall be certified by the teachers on the certificates of attendance.

2. The Committee on Education likewise recommended that every student should be obliged to serve the offices of clerk and dresser; this was done with a full knowledge that a rearrangement of hospital duties would have to be made, but with a conviction that the practical instruction of an hospital should be made available to all, and that, if it is not done, it is impossible that the great body of students can acquire even the rudiments of practical hospital work. The College of Surgeons of England has had for some years a regulation of this kind, and the Society of Apothecaries of London has now ordered that after June 1870 any candidate for its license shall have served for at least six weeks the office of clerk.

A very competent authority has pronounced this recommendation of the Committee on Education to be the most important in their whole Report; and in the belief that this is true, and considering that, like the former order of the Society of Apothecaries, it must now be carried out in all the English schools, it is recommended that the Council should make it current throughout the United Kingdom.

3. The Committee on Education advised that the teaching of Therapeutics should be separated entirely from that of Pharmacy, and should form the subject of a later course. Although this point has not been yet fully discussed by the Licensing Bodies, yet, as it has been advocated by every teacher of Materia Medica whose replies are in the Appendix to the Education Report, and is supported by Dr Christison and Dr Aquilla Smith, as well as by many of the teachers of the Principles of Medicine, there can be no doubt that it would be an essential improvement, and should be carried into effect without delay.

4. The last point on which the Council can now properly take action is more connected with examinations than method of teaching, yet has a close bearing upon this.

The Clinical Examination in Medicine and Surgery is now carried out by so many Licensing Bodies, and is so good a test of practical knowledge, that the Council ought to make it imperative. It is believed that this proposition will command general assent in and out of the Council, and there is therefore no occasion to enter more fully into the argument in its favour.

As the early session of Council has rendered it impossible to put on record the replies of all the Licensing Bodies to the Education Report, it will be desirable to reappoint the present Committee in order that a final Report, disposing of all the other matters now necessarily left untouched, may be made at a future session of Council. (Signed) E. A. PARKES, *Chairman.*

The foregoing Report was ordered, on the 16th of February, to be circulated amongst the Members of Council; after that date Reports were received from the Royal College of Surgeons of England, the Universities of Edinburgh, Aberdeen, and Glasgow, the Royal College of Physicians of Edinburgh, the Royal College of Surgeons of Edinburgh, and the Queen's University in Ireland. These documents arrived at too late a date to be referred to in the Report.

It was then moved by Dr Parkes, and seconded by Mr Hargrave—"That the Report be received and entered on the Minutes, and that the Appendices, and the Reports which arrived after the Appendices were printed, be distributed to the Members of Council."

And an amendment, moved by Dr Alexander Wood, and seconded by Sir Dominic Corrigan—"That the Report as now laid before the Council is necessarily incomplete, as it does not include documents which have been received since it was drawn up, and that it be remitted to the Committee for the purpose of being brought down to the present date,"—was negatived.

Another amendment, moved by Dr Rumsey, and seconded by Dr Alexander Wood—"That the Report be continued on the Programme until Friday, when it should be taken up in connexion with the Lord President's letter, and that power be given to the Committee to make any alterations and additions that may appear to them necessary,"—was also negatived. The motion was then put to the vote and carried.

The joint Report of the Executive and Finance Committees on the best means of lessening the expenditure of the Council was then read; we print it as finally adopted by the Council.

The joint Committees recommend that the duration of the annual session of the General Council be limited by standing order to six days; the remaining business, so far as delegated by the General Council, to be transacted by the Executive Committee; the meetings of the Executive Committee for that purpose being held immediately after the termination of the session of the General Council.

### PRINTING.

*The Register.*—The duty of publishing the Register occasions a large yearly expense to the Council. The net cost of the Register and Quarterly Lists (after deducting moneys received) was, last year, £394, 3s. 6d. The 2000 copies supplied to Government cost more than is paid for them by 1s. 5d. per copy. The Committees recommend that the Executive Committee, with the Treasurers, consult with the Comptroller of Her Majesty's Stationery Office, with the view of reducing the expense. They also recommend that only one Supplementary List be published annually, viz., at midsummer, instead of the four Quarterly Lists.

*Reports.*—The Committees recommend that Chairmen of Committees be directed to give, in most cases, in abstract only, the various documents and other evidence on which their reports are founded; it being understood that in all cases the original documents will be preserved for reference; and, that Reports drawn up when the General Council is not sitting, be printed only by direction of the President and Treasurers.

*Minutes, etc.*—A considerable saving has been effected in the printing of the Minutes and the Annual Volume by the operation of the financial recommendations of last year. Nevertheless, those expenses are still very large. The Committees therefore advise that the Business Committee and the Council be vigilant in limiting as far as possible the quantity of documentary matter to be printed in the Programmes and the Minutes.

The Committees are of opinion that the printing of the volumes of Minutes should be under the direction of the Executive Committee.

The List of Examining Bodies whose examinations fulfil the conditions of the Medical Council, which had been prepared by the Examination Committee, was laid before the Council and approved of.

It was the same as the list of last year, except that the following notice was inserted at the head of the list, viz.:—

"A Degree in Arts, of any University of the United Kingdom, or of the Colonies, or of such other Universities as may be specially recognised from time to time by the Medical Council, may be accepted, provided always, that the examination for the degree has included all the subjects recognised by the General Medical Council."

Some correspondence which had passed between the Registrar and the Secretary of the Queen's University, Dublin, was then read, as was also the following:—

### *Preliminary Report of the Medical Committee of the Queen's University, presented to the Senate on the 2d December* 1869.

The Medical Committee appointed by the Senate to consider and report upon the Medical Curriculum, and especially upon the time for holding the Preliminary Examination of Medical Students, and the correspondence with the Medical Council, have held two meetings, which were chiefly occupied with a discussion on the latter part of the subject-matter referred to them. The Medical Committee beg to report to the Senate as follows:—

The amendment of the Medical Curriculum of the Queen's University in any particular in which it may be deemed susceptible of improvement, will demand a further and careful consideration of the Committee, and their conclusions will be laid before the Senate at a further meeting.

The Medical Committee are meanwhile strongly of opinion that a simple Preliminary Examination, adapted as it must be to the early age at which students commence their professional career, is not of itself an adequate guarantee of extra-professional knowledge. They consider a further course of study in Arts essential to secure the full advantages of the liberal as well as the professional education which should be required from candidates for degrees in a University. This principle the Queen's University has consistently and steadily acted upon, and its requirements have always been in excess of those of the General Medical Council.

The Committee further recommend that the Secretary inform the General Medical Council that the Senate are engaged in revising the whole Scheme of Medical Education and Examination, and that the question of holding a Preliminary Examination prior to the commencement of medical studies, will obtain their careful consideration. (Signed) G. JOHNSTONE STONEY.

All of which were ordered to be entered on the Minutes.

The usual returns from the India Office of Examinations for Medical Commissions were then laid upon the table, and ordered to be entered on the Minutes.

*Statement of the Degrees, Diplomas, and Licenses of the Candidates for Commissions in the Medical Department of the Indian Army, who, in February* 1869, *presented themselves for Examination; showing the number that passed, and did not pass, distinguishing the Qualifications, both Medical and Surgical, under the heads of the several Licensing Bodies.*

| NAMES OF LICENSING BODIES. | | QUALIFICATIONS. | | | | |
|---|---|---|---|---|---|---|
| | | Number of Qualifications. | | | Deficient in | |
| | | Total. | Number passed. | Number failed. | Medicine. | Anatomy, Operative Surgery, and Surgery. |
| Royal Coll. of Physicians, London . | Licentiates | 1 | 1 | ... | ... | ... |
| Royal Coll. of Surgeons, England . | Members | 12 | 9 | 3 | ... | ... |
| Society of Apothecaries, London . | Licentiates | 9 | 7 | 2 | ... | ... |
| Royal Coll. of Physicians, Edinburgh, | Licentiates | 10 | 5 | 5* | ... | ... |
| Royal Coll. of Surgeons, Edinburgh, | Licentiates | 11 | 6 | 5* | ... | ... |
| University of Edinburgh | Bachelor of Medicine | 2 | 2 | ... | ... | ... |
| | Doctor of Medicine . | 1 | ... | 1 | ... | ... |
| University of Glasgow . | Master in Surgery . | 1 | 1 | ... | ... | ... |
| Do. Aberdeen | Bachelor of Medicine | 3 | 1 | 2 | ... | ... |
| | Master in Surgery . | 2 | 1 | 1 | ... | ... |
| University of London . | Doctor of Medicine . | 1 | 1 | ... | ... | ... |
| | Bachelor of Medicine | 2 | 2 | ... | ... | ... |
| University of Dublin . . | Bachelor of Medicine | 3 | ... | 3 | ... | 1 |
| | Master in Surgery . | 4 | 1 | 3 | | |
| Apothecaries' Hall, Dublin . . . . | Licentiates | 1 | 1 | ... | ... | ... |
| King&Queen'sColl.Physicians,Ireland, | Licentiates | 4 | 1 | 3 | ... | ... |
| Royal College of Surgeons, Ireland . | Licentiates | 4 | 1 | 3 | ... | ... |
| Queen's University, Ireland . . . . . . . | Doctor of Medicine . | 7 | 3 | 4 | ... | ... |
| | Master in Surgery . | 5 | 2 | 3 | ... | ... |
| | Total | 83 | 45 | 38 | ... | 1 |

* Bad in all subjects.

CANDIDATES—Successful, 20; failed, 18. Total, 38.
DIPLOMAS AND DEGREES—Successful, 45; failed, 38. Total, 83.

N.B.—Of the 18 Candidates returned as unsuccessful, 16 were qualified, but were not accepted, as only 20 appointments were made.

*Statement of the Degrees, Diplomas, and Licenses of the Candidates for Commissions in the Medical Department of the Indian Army, who, in August* 1869, *presented themselves for Examination; showing the number that passed, and did not pass, distinguishing the Qualifications, both Medical and Surgical, under the heads of the several Licensing Bodies.*

| NAMES OF LICENSING BODIES. | | QUALIFICATIONS. | | | | |
|---|---|---|---|---|---|---|
| | | Number of Qualifications. | | | Deficient in | |
| | | Total. | Number passed. | Number failed. | Medicine. | Anatomy, Operative Surgery, and Surgery. |
| Royal Coll. of Physicians, London | Licentiates | 2 | 2 | ... | ... | ... |
| Do. Surgeons, England | Members | 11 | 11 | ... | ... | ... |
| Society of Apothecaries, London | Licentiates | 7 | 7 | ... | ... | ... |
| Royal Coll. of Physicians, Edinburgh | Do. | 7 | 7 | ... | ... | ... |
| Do. Surgeons do. | Do. | 4 | 4 | ... | ... | ... |
| University of Edinburgh | Bachelor of Medicine | 1 | 1 | ... | ... | ... |
| Do. do. | Master in Surgery | 1 | 1 | ... | ... | ... |
| Do. Aberdeen | Bachelor of Medicine | 3 | 3 | ... | ... | ... |
| Do. do. | Master in Surgery | 3 | 3 | ... | ... | ... |
| Do. Glasgow | Bachelor in Medicine | 1 | 1 | ... | ... | ... |
| Do. do. | Master in Surgery | 1 | 1 | ... | ... | ... |
| Do. Dublin | Bachelor of Medicine | 8 | 7 | 1* | ... | ... |
| Do. do. | Master in Surgery | 8 | 7 | 1* | ... | ... |
| King and Queen's Coll. of Physicians, Ireland | Licentiates | 4 | 4 | ... | ... | ... |
| Royal Coll. of Surgeons, Ireland | Do. | 7 | 7 | ... | ... | ... |
| Queen's University of Ireland | Doctor in Medicine | 6 | 6 | ... | ... | ... |
| Do. do. do. | Master in Surgery | 4 | 4 | ... | ... | ... |
| Apothecaries' Hall, Dublin | Licentiates | 2 | 2 | ... | ... | ... |
| University of London | Bachelor of Medicine | 1 | 1 | ... | ... | ... |
| Faculty of Physicians and Surgeons, Glasgow | | ... | ... | ... | ... | ... |
| Total number of Qualifications | | 82 | 80 | 2 | ... | ... |

* Deficient in all subjects.

CANDIDATES—Successful, 39; failed, 1. Total, 40.
DIPLOMAS AND DEGREES—Successful, 80; failed, 2. Total, 82.

The following Report of a Visitation of Examinations at the Queen's University in Ireland was then read:—

It being an instruction from the General Medical Council, at its last session, that the Visitation of the Professional Examinations should be continued during another year, I attended those of the Queen's University, held on the 7th, 8th, and 11th of October 1869, upon which occasions there were thirty-one candidates present.

The examinations were similar in character, and were conducted after the same manner as stated in my former reports, but with this important difference, that the candidates for the degree of M.D. were examined at the bedside of patients in the South Dublin Workhouse Hospital, where several interesting cases were submitted for diagnosis, while the candidates for the degree of M.Ch. were examined at the College of Surgeons on cases of surgical disease, selected from the admission-room of hospitals: and with regard to facilities for Clinical Examinations in Dublin, it is noteworthy, I think, that many of the hospitals are now freely thrown open for the Pass Examination of candidates.

I cannot, however, conclude this Report without remarking, that there is yet need of a stricter rule being applied for passing the Preliminary or Arts course, and also that candidates for the degrees in the Faculty of Medicine should undergo fuller and more extended *vivâ voce* Examinations.

10th February 1870. (Signed) CHARLES HENRY LEET, M.D.

This Report was ordered to be entered on the Minutes.

It was then agreed that the Lord President of the Privy Council should be applied to for a copy of the Bill introduced by Sir John Gray.

*On Friday, 25th February*, the Council met at one o'clock, the President, Dr Paget, in the chair. The President stated that he had been unable to procure a copy of Sir John Gray's Bill from the Lord President of the Privy Council, as it had not been printed. The Council then proceeded to consider the letter from the Lord President of the Privy Council which had been read the previous day, and the whole day was spent in discussing its provisions, and the various motions resulting from this discussion. This was continued next day, *Saturday, 26th February;* and ultimately the following motion, moved by Dr Thomson, and seconded by Dr Bennett, was carried by a majority, seventeen voting for it, one against it, and four declining to vote :—

"That this Council is of opinion that a joint Examining Board should be formed in each of the three divisions of the kingdom; and that every person who desires to be registered under any of the qualifications recognised in Schedule (A) to the Medical Act shall be required previously to such registration to appear before one of these Boards, and be examined on all the subjects which may be deemed advisable by the Medical Council; the rights and privileges of the Universities and Corporations being left in all other respects the same as at present."

*Majority*, 17.—The President, Dr Bennett, Mr Hawkins, Mr Cooper, Dr Acland, Dr Humphry, Dr Embleton, Dr Storrar, Dr Andrew Wood, Dr Fleming, Dr Thomson, Mr Hargrave, Dr Leet, Dr Apjohn, Dr Sharpey, Dr Parkes, Dr Stokes.

*Minority*, 1.—Sir D. Corrigan, Bart.

Declined to vote:—Dr Alexander Wood, Dr Macrobin, Dr A. Smith, Dr Rumsey.

On *Monday, 28th February*, the Council passed the following resolutions in reference to the proposed conjoint Examining Boards:—

"That, in accordance with the foregoing resolution, the Universities and Medical Corporations established in each division of the United Kingdom shall be requested to concert a scheme for the constitution and regulation of a conjoint Examining Board for that part of the kingdom to which they belong, and shall, on or before June 1, 1870, transmit such scheme to the consideration of the General Medical Council."

"That any alterations in the aforesaid schemes deemed necessary by the Council should be considered by the conjoint Bodies, and their opinion reported to the Council; and that, in case of disagreement between any of the conjoint Bodies and the Council, the points of difference should be referred to the consideration and decision of Her Majesty's Privy Council. That the same course should be followed if, in process of time, it should be considered advisable to make any alteration in the original constitution or rules of the conjoint Examining Boards."

"That the powers required for carrying the foregoing resolutions into operation be sought from the Legislature; and that Her Majesty's Government be requested to bring in and carry through Parliament a Bill to amend the Medical Acts, which shall contain the requisite provisions."

Various other less important resolutions on the same subject were then passed, and the Executive Committee were entrusted with the conduct of the business necessary to the carrying out of these resolutions. A motion by Sir D. Corrigan for a preliminary inquiry by a Royal Commission was negatived after a short debate.

The Council then entered on the consideration of the Report of the Committee appointed by the Council to confer with the Licensing Bodies on the Education Report, read at the meeting of the Council on the 24th inst., and it was moved by Dr Parkes, seconded by Mr Hawkins, and agreed to:—"That the Committee appointed to confer with the Licensing Bodies on the Report of the Committee on Education be reconstituted, and be termed the Committee on Professional Education."

The Council next took into consideration the Report of the Queen's University in Ireland relative to Preliminary Examination, read at the meeting of the Council on the 24th inst., when a motion was made by Dr Parkes, that the Privy Council be urged to refuse registration to the graduates of the Queen's University till the University comply with the recommendation of the Medical Council on the preliminary examination of medical students.

The consideration of this matter, having been adjourned, was resumed on

*Tuesday*, 1*st March*, and resulted in Dr Parkes's motion being withdrawn in favour of one by Dr Alexander Wood, to postpone action for the present.

The President submitted to the Council the draft of a letter to the Lord President of the Privy Council, prepared by the Committee appointed for that purpose. The letter was considered, paragraph by paragraph, and adopted as follows:—

General Council of Medical Education and Registration,
32 Soho Square, London, W., 1st March 1870.

SIR,—I beg leave to acquaint you, for the information of the Lord President of Her Majesty's Council, that your letter of the 2d February, written by direction of his Lordship, the receipt of which I have already acknowledged, seemed to me to deal with matters of so much importance, and to require so early a reply, that I thought it advisable to summon without delay a meeting of the General Medical Council for its consideration.

The General Medical Council desires me to express the great satisfaction with which it received the intimation conveyed in your letter, that the Lord President would hope to propose to Parliament a measure for amending the Medical Act, which should remedy not only such defects as were touched on by the Medical Council's Draft Bill of last session, but effect much larger and more important improvements in the system under which persons now receive licenses to exercise the profession of medicine.

In addition to former communications from the Medical Council relative to the matters requiring legislation in any new Medical Act, I now beg to forward, for the information of the Lord President, copies of resolutions agreed to by the Medical Council at its meetings on the 26th and 28th February 1870, when your letter was brought under its consideration.

You will see from these resolutions that the Medical Council agrees with the Lord President in thinking that the present system of medical examination entitling to registration requires amendment, and that the Council considers that some more or less consolidated examining authority should be instituted.

The Council has decided that this may be best accomplished by the formation of a Conjoint Examining Board in each division of the kingdom, such Board being constituted by a combination of the present Licensing Bodies on a plan to be hereafter determined.

The Council believe that arrangements may be thus made, which, with the least injury to existing Institutions, will ensure that the examination for license to practise shall be in every respect complete and trustworthy.

The Council has appointed its Executive Committee to confer with the Lord President, and to bring before him, for his consideration, the various provisions which it would be necessary to introduce into an amended Act, in order to carry out the objects stated in this and former communications from the Council.

I have only further to acquaint you, that the Executive Committee will be prepared to attend upon his Lordship at any time that he may appoint.—I have the honour to be, Sir, your most obedient servant,

(Signed) GEORGE EDWARD PAGET, M.D.,
*President of the General Medical Council.*

John Simon, Esq., F.R.S., Medical Officer to the Privy Council.

It was then moved by Dr Parkes, seconded by Dr Andrew Wood, and agreed to:—"That this letter, as now approved of, be transmitted, with the Minutes of the Council during its present session, and the other official communications on the subject of an amended Medical Act, to the Lord President of Her Majesty's Council."

The Report of the State Medicine Committee was then presented by Dr Ackland; its most important contents were the following clauses, suggested by Mr Ouvry, as proper to be inserted into an amended Medical Act, in respect of a State Medicine qualification:—

*Addition to Preamble.*—And whereas it [may be] expedient that Degrees or Qualifications in "State Medicine" should be recognised, and that the General Medical Council should be authorized to register such Degrees or Qualifications.

Clause A.—"State Medicine" shall include the following subjects, or such of them, or such other subjects as shall be determined upon by the Bodies or Body which shall be authorized to grant Degrees or Licenses in "State Medicine," viz., Forensic Medicine, Toxicology, Morbid Anatomy (Human and Comparative), Pyschological Medicine, Laws of Evidence, Preventive Medicine, Vital and Sanitary Statistics, Medical Topography, and portions of Engineering Science and Practice.

Proposed Amendment.—*The expression "State Medicine" shall be taken to mean the application of the Physical and Medical Sciences to the safety and well-being of the community; and it shall include Medical Jurisprudence and the care of the Public Health.*

Clause B.—That if, at any time hereafter, any University of the United Kingdom, or any Body in the United Kingdom now authorized, or which shall hereafter be authorized, to grant Licenses in Medicine or Surgery, shall hereafter, either by virtue of the powers now vested in them, or any of them, or by virtue of any power to be conferred upon them, or any of them, by Act of Parliament or by Charter, or any legal means, grant any Degree or License in State Medicine, it shall be lawful for the General Medical Council to insert such Degree or License in the Medical Register as a qualification, in addition to any other qualifications mentioned in the Medical Acts.

Proposed Amendment.—*That if, at any time hereafter, any University of the United Kingdom shall grant any Degree or Certificate of Proficiency or University Title in State Medicine, it shall be lawful for the General Medical Council to insert such Degree, Certificate, or Title in the Medical Register as a Qualification, in addition to any other Qualifications in the Medical Acts.*

Clause C.—*That if, at any time hereafter, any two or more of the Licensing Bodies in the United Kingdom, mentioned in the Schedule (A) to the Medical Act,* *not being Universities, shall, with the sanction and under the direction of the General Medical Council, unite in conducting, or in providing for, the Examinations to be required for a Qualification in State Medicine, and shall be empowered by Act of Parliament, or by Charter, or by any other legal means, to grant a License in State Medicine, and shall thereupon (proceed to) grant the same, it shall be lawful for the General Medical Council to insert such License in the Medical Register as a Qualification, in addition to any other Qualifications mentioned in the Medical Acts.*

The Committee was reappointed, with orders to confer again with Mr Ouvry, and report to the next meeting of Council; the opinions of the Universities and Corporations in regard to State Medicine being ordered to be printed as an Appendix to that Report.

The Treasurer's Report was then laid before the Council, and adopted without a division; it showed an excess of expenditure over income of £765.

| | *Estimated Income for the Year* 1869. | | | | | | *Actual Income for the Year* 1869. | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | £ | s. | d. | £ | s. | d. | £ | s. | d. | £ | s. | d. |
| Fees received by— | | | | | | | | | | | | |
| Branch Council for England | 2450 | 0 | 0 | | | | 2281 | 15 | 0 | | | |
| " " Scotland | 700 | 0 | 0 | | | | 617 | 10 | 0 | | | |
| " " Ireland | 800 | 0 | 0 | | | | 732 | 0 | 0 | | | |
| | | | | 3950 | 0 | 0 | | | | 3631 | 5 | 0 |
| Dividends received by— | | | | | | | | | | | | |
| Branch Council for England | 670 | 0 | 0 | | | | 644 | 18 | 9 | | | |
| " " Scotland | 65 | 0 | 0 | | | | 64 | 10 | 10 | | | |
| " " Ireland | 60 | 0 | 0 | | | | 64 | 2 | 0 | | | |
| | | | | 795 | 0 | 0 | | | | 773 | 11 | 7 |
| Sale of Registers . . . | ... | | | 280 | 0 | 0 | ... | | | 271 | 7 | 6 |
| Penalties . . . . | ... | | | ... | | | ... | | | 47 | 14 | 6 |
| | | | | £5025 | 0 | 0 | | | | £4723 | 18 | 7 |
| | *Estimated Expenditure for the Year* 1869. | | | | | | *Actual Expenditure for the Year* 1869. | | | | | |
| | £ | s. | d. | £ | s. | d. | £ | s. | d. | £ | s. | d. |
| Expenses of— | | | | | | | | | | | | |
| General Council . . . | ... | | | 4300 | 0 | 0 | ... | | | 4265 | 13 | 9 |
| Branch Council for England | 700 | 0 | 0 | | | | 642 | 9 | 10 | | | |
| " " Scotland | 800 | 0 | 0 | | | | 286 | 0 | 7 | | | |
| " " Ireland | 300 | 0 | 0 | | | | 295 | 9 | 10 | | | |
| | | | | 1300 | 0 | 0 | | | | 1224 | 0 | 3 |
| | | | | 5600 | 0 | 0 | Total Expend. | | | 5489 | 14 | 0 |
| | Estimated Income | | | 5025 | 0 | 0 | Total Income | | | 4723 | 18 | 7 |
| | Estimated Excess of Expenditure | | | £575 | 0 | 0 | Excess of Expendit. over Income | | | £765 | 15 | 5 |

The Council having balloted for the Executive Committee, the following were found to be elected:—Dr Bennett, Mr Hawkins, Dr Acland, Dr Andrew Wood, Dr A. Smith, Dr Sharpey.

The Pharmacopœia Committee was reappointed, and consists of Dr Christison, Dr Sharpey, Dr A. Smith, Dr Quain.

The following Resolution passed by the Branch Council for England, on the 16th December 1869, and the communication which follows, from the Clerk of the Society of Apothecaries, were then read, viz.:—"That, in the opinion of this Branch Council, it is expedient that the Society of Apothecaries of London should possess the power of striking off from their List the names of any of their Licentiates who may have been guilty of infamous conduct; and that Mr Cooper be requested to communicate this opinion to the Society, and to inquire whether the Society now possesses, or can obtain, such power."

Apothecaries' Hall, London, E.C., January 15, 1870.

SIR,—I am directed by the Master of the Society to acknowledge the receipt of a Resolution of the Branch Council, respecting its opinion, that it is expedient that the Society of Apothecaries should possess the power of striking off from their List the name of any of their Licentiates who may have been guilty of infamous conduct, and desiring to know whether the Society now possesses, or can obtain, such power.

The Society entirely concur in the opinion expressed by the Branch Council on this subject.

Unfortunately the Society do not possess the power in question, and can only obtain it by legislative enactment.

The Society would willingly avail themselves of any opportunity which presented itself for acquiring such a power, and would be grateful for any assistance which the Branch Council, or the General Medical Council, could render them with a view to its attainment.—I have the honour to be, Sir, your most obedient servant, (Signed) ROBT. B. UPTON, *Clerk to the Society.*

Dr Hawkins, Registrar, General Medical Council.

The Resolution of the English Branch Council, and the communication from the Society of Apothecaries, were ordered to be entered on the Minutes.

And it was moved by Mr Hawkins, seconded by Mr Cooper, and agreed to :—"That, in the opinion of this Council, it is desirable that all Medical Corporations should possess the power, legally exercised by some of them, of striking off from their Lists the name of any person registered under the Medical Act, under any of the Qualifications conferred by those Bodies, who shall have been convicted in England or Ireland of any felony or misdemeanour, or in Scotland of any crime or offence, or shall, after due inquiry, be judged by any one of those Bodies, or by the General Medical Council, to have been guilty of infamous conduct in any professional respect; and that a clause to this effect should be introduced into any amending Medical Act."

The Council agreeing that the foregoing Resolution should be transmitted to the Lord President of the Council.

Various other communications of minor importance were then read, and, with the usual complimentary votes, this short, though important, session was brought to a close.

## PUBLICATIONS RECEIVED.

James Adams, M.D.,—Biographical Sketch of the late Professor Penny. Glasgow, 1870.

Julius Althaus, M.D.,—Treatise on Medical Electricity. London, 1870.

W. R. Basham, M.D.,—Renal Diseases. London, 1870.

James George Beaney, F.R.C.S.,—Syphilis popularly considered. Melbourne, 1869.

Prof. G. B. Crivelli e L. Maggi,—La Derivazione dei Vibrio-bacillus. Milan, 1869.

Nouveau Dictionnaire de Médecine et de Chirurgie Pratiques, vol. xii. Paris, 1870.

Horace Dobell, M.D.,—Progress of Practical and Scientific Medicine. London, 1870.

Durham Dunlop,—The Philosophy of the Bath. Dublin, 1870.

Frederick Edwards, jun.,—Our Domestic Fireplaces. London, 1870.

Charles Elam, M.D.,—Medicine, Disease, and Death. London, 1870.

Charles Alex. Gordon, M.D.,—Army Surgeons and their Works. London, 1870.

S. D. Gross, M.D.,—Memoir of Robley Dunglison, M.D., LL.D. Philadelphia, 1869.

Guy's Hospital Reports, vol. xv. London, 1870.

Stanley Haynes, M.D.,—Voluntary Patients in Asylums. Lewes, 1870.

Robert Gardiner Hill, F.S.A.,—Lunacy: Its Past and its Present. London, 1870.

James Hunt, Ph. D.,—Stammering and Stuttering. London, 1870.

Joseph O. Hutchison, M.D.,—Acupressure. Albany, 1869.

Thomas Inman, M.D.,—On the Restoration of Health. London, 1870.

J. H. James, F.R.C.S.—Paracentesis of the Bladder above the Pubes. London, 1870.

C. Handfield Jones, M.B.,—Studies on Functional Nervous Disorders. London, 1870.

H. Knapp, M.D.,—Treatise on Intraocular Tumours. Translated by S. Cole, M.D. New York, 1869.

E. Ray Lankester, B.A.,—Comparative Longevity in Man and the Lower Animals. London, 1870.

H. Letheby, M.B., Ph.D.,—On Food. London, 1870.

P. Lorain,—Etudes de Médecine Clinique: Le Pouls. Paris, 1870.

Map of the Geographical Distribution of the Medicinal Substances in the British Pharmacopœia of 1867. London, 1870.

E. D. Mapother, M.D.,—The Body and its Health. Dublin, 1870.

Selby Norton, M.D.,—Infantile Diseases. London, 1870.

Joseph C. Richardson, M.D.,—Diagnostic Value of the Corpuscular Blood Elements in the Urine of Bright's Disease. Philadelphia, 1870.

James Rogers, M.D.,—The Present State of Therapeutics. London, 1870.

J. von Rönne,—Die Criminalistiche Zurechnungsfähigkeit. Berlin, 1870.

Jas. B. Russell, M.D.,—Hydrate of Chloral as a Hypnotic in Typhus. Glasgow, 1870.

Richard Schmitz, M.D.,—Water and Baths of Neuenahr. London, 1870.

F. C. Skey, C.B.,—Lectures on Hysteria. London, 1870.

Frederick G. Snelling, M.D.—Relaxation of the Pelvic Symphyses during Pregnancy and Parturition. New York, 1870.

T. Gaillard Thomas, M.D.,—History of Nine Cases of Ovariotomy. New York, 1869.

Transactions of the American Ophthalmological Society. New York, 1869.

Transactions of the Pathological Society of London, vol. xx. London, 1869.

James Tyson, M.D.,—The Cell Doctrine. Philadelphia, 1870.

Zepherina P. Veitch,—Handbook for Nurses for the Sick. London, 1870.

Henry W. Williams, M.D.—Diseases of the Eye. Boston, 1869.

Erasmus Wilson, F.R.C.S.,—Lectures on Ekzema. London, 1870.

## PERIODICALS RECEIVED.

American Journal of Insanity,—January.

American Journal of the Medical Sciences,—January.

American Journal of Obstetrics,—November.

American Practitioner,—January.

Annales d'Oculistique,—Jan., Feb.

Berlin. Klin. Wochenschrift,—Nos. 5 to 12.

Births, Deaths, and Marriages, Monthly Return of,—January, February; Quarterly Return,—December.

Boston Medical and Surgical Journal,—March 10.

British Medical Journal,—November 27, 1869, to March 19, 1870.

Bulletin Général de Thérapeutique,—February, March.

Dublin Quarterly Journal of Medical Science,—February.

Food Journal,—February, March.

Gaceta Médica de Granada,—Nos. 1, 2.

Gazette des Hôpitaux,—Nos. 9 to 34.

Gazette Hebdomadaire de Médecine, etc.,—Nos. 4 to 11.

Gazette Médicale de Paris,—Nos. 5 to 12.

Glasgow Medical Journal,—February.

Journal de Médecine Mentale,—January.

Journal of the Royal Agricultural Society of England,—No. 11.

Journal of the Scottish Meteorological Society,—January.

Klin. Monatsblät. für Augenheilkunde,—August, September.

Madras Monthly Journal of Medical Science,—January.

Medical Press,—Feb. 2 to March 23.

Medical Times and Gazette,—November 27, 1869, to March 19, 1870.

Nederlandsch Tijdschrift voor Geneeskunde,—April to November 1869; Aflevering 3 and 4.

New Orleans Journal of Medicine,—October, January.

New York Medical Journal,—February, March.

Philadelphia Medical and Surgical Reporter,—January 8 to 29.

Practitioner,—February, March.

Revue de Thérapeutique Médico-Chirurgicale,—Nos. 3 to 6.

Virchow's Archiv,—December, January.

Wochenblatt der Aerzte in Wien,—No. 48, 1869; Nos. 3 to 5, 1870.

# Part First.

## ORIGINAL COMMUNICATIONS.

### ARTICLE I.—*Notes of an Epidemic of Typhoid Fever at Donaldson's Hospital.* By Dr GILLESPIE, Pres. R.C.S.E., etc.

(*Read before the Medico-Chirurgical Society of Edinburgh, 2d March* 1870.)

DONALDSON'S HOSPITAL has been open for the education and maintenance of poor children for twenty years, and no case of continued fever has ever occurred there till last autumn, when an epidemic of typhoid fever broke out within its walls. Standing, as it does, in an elevated position, and in a salubrious locality, apart from all noxious influences, with excellent facilities for good drainage, its immunity from fever was not surprising; but when a number of its young inmates were laid prostrate by typhoid fever during the last three months of 1869, it seemed evident that some disturbing causes were at work to poison the hitherto atmospheric purity of the building.

As these causes have been most satisfactorily discovered, and efficient means employed for their arrestment, it appears to me that it would not be devoid of interest to my medical brethren to give a short sketch of the origin, progress, and termination of the malady, with a more detailed account of the hygienic, or rather want of hygienic, conditions which caused the development of the disease.

Typhoid fever is now so thoroughly understood that it would be a waste of time to describe at any length individual cases; but where so many patients are grouped together in one building, the opportunity is afforded for valuable observation, and I have therefore noted down whatever struck me as worthy of record during my visits at the hospital.

At the time of the outbreak there were 227 children in the institution and 29 adults. The proportions were:—157 hearing children—viz., 85 boys and 72 girls; 70 deaf mutes—viz., 40 boys and 30 girls. Of these, 31 children and 2 adults suffered from typhoid fever.

The premonitory or incubation symptoms in the children were wonderfully similar—so much so that the matron, teachers, and nurses were able to pick out, at an early period, from the throng of children those under the influence of the fever.

These symptoms were loss of appetite, languor, and headache, the pain almost invariably referred to the back of the head, the tongue red at the tip and edges, with white loaded centre and elevated papillæ, soon assuming, however, in the worst cases, the dry brown characteristic febrile appearance. During the early stage there was no tendency to diarrhœa, but the fever was very high, attended in almost every case by delirium of the sthenic rather than the asthenic type.

The fever was of long duration, with no distinct crisis, and it left the patients very weak and exhausted.

The rose-coloured spots were noticed in about half the cases, it being observed that those children with dingy thick skins had no spots, or at all events that they could not be discerned.

Diarrhœa was a frequent symptom in the advanced stages, but was amenable to treatment, chlorodyne or lime-water being found sufficient to keep it in check.

In several of the children, hæmorrhage from the nose repeatedly occurred. Most of the hearing children became temporarily deaf.

Two children relapsed after becoming apparently convalescent; both deaf mutes—one a boy, the other a girl.

The case of the boy, William Gibson, was a most interesting one, and for many weeks he caused me much apprehension as to the chance of his ultimate recovery.

He was seized with fever on 2d December, and he is still, at this date, 19th February, confined to bed from extreme prostration, though all fears for his safety are at an end.

The fever and delirium were from the first very high, but he soon became extremely weak and exhausted, and required active stimulation to keep him from sinking.

On the 21st day of the fever he had a sort of crisis, sweating very profusely, and for a week he was evidently tending to convalescence; but a relapse occurred, and he became more dangerously ill than at first. A most unfortunate complication ensued, for he completely lost his eyesight. There was nothing apparently wrong, and he was too ill to examine him by the ophthalmoscope, but it was evident he was utterly blind. Total blindness continued for about six weeks, but now that convalescence is becoming established the sight is returning.

Towards the end of January, when the boy's condition was at its worst, and we were daily expecting death to terminate his sufferings, I found him with symptoms of congestion of the lungs and serous effusion in the pleuræ; and to relieve the extreme and urgent dyspnœa, I ordered a mustard-blister to the chest. It was only kept on for a quarter of an hour, but his vitality was so low that a great part of the skin sloughed. The breathing was, however, improved, and by constant watching, and the unsparing use of stimulants, he passed gradually through the imminently dangerous stage.

Several other cases, by the severity of their symptoms, occasioned anxiety for a short time, but all ultimately got well.

One of the girls, Mary Maconochie, a deaf mute, caught hooping-cough soon after her admission, with fever, owing to there being several cases of that disease in the sick department of the hospital. She was for a long time very seriously ill, and, after being convalescent for about a week, relapsed. She again managed to emerge from the fever, but is now (19th Feb.) suffering from rheumatic swelling of the joints.

One of the hearing boys, James Milroy, who, some months previously, had hurt his shoulder in throwing a cricket-ball, and which, from the symptoms at the time of the accident, seemed to be an injury to the upper epiphysis of the humerus, had, during convalescence from the fever, a very acute attack of inflammation of the upper end of the bone, which ultimately involved the joint, and which threatened to suppurate, but under rest and repeated blistering, the untoward symptoms subsided, and his arm is nearly well.

I mentioned that the premonitory symptoms in the children were invariably the same, viz., languor and headache, without diarrhœa; as regards one of the adults, a young housemaid, the same train of symptoms was present, but the other, a teacher of the deaf mutes, suffered for about a fortnight from obstinate sickness and diarrhœa, before the febrile symptoms became high, or the rose-coloured spots appeared. This gentleman, though not apparently much exposed to the atmospheric causes of the fever, had expressed much dread of its invasion. The housemaid was much employed in the corridor and dormitories on the boys' side, where the fever became prevalent. She was the second patient attacked, coming under my care on the 25th of October last. She was a somewhat delicate-looking girl, of a lymphatic temperament.

The course of the epidemic corroborated the view I have always held that typhoid fever is not contagious. The nurses slept with immunity for upwards of three months in rooms lying between, and communicating by folding-doors with, the wards occupied by fever. The disease was not confined to certain dormitories; almost every dormitory in the house, as will be hereafter shown, furnished cases; while the adults, with the exception of the two already mentioned, escaped, because they were not much exposed to the noxious influences which caused the fever, though they were brought into daily contact with the children.

The first case of fever was discovered on the 25th October 1869, and one or two fresh cases were almost daily sent to the sick wards up to 17th December, when the last case was admitted.

As no child was dismissed from the sick wards convalescent until 6th December, it is evident that it could not be infection conveyed by children suffering from the fever that propagated the disease.

The treatment may be said to have consisted in letting well alone, except when special symptoms required to be combated. Mild diaphoretics were employed at the early stage of the fever, except when they were contraindicated by the presence of sickness or diarrhœa. Marked benefit was observed from the employment of bromide of potass during delirium, while chlorodyne was the only narcotic used, and that rarely, for the arrestment of diarrhœa. Wine and brandy were only necessary in the worst cases, but were then invaluable. During convalescence, wine and quinine seemed to assist in restoring the strength.

By the 3d of January, all the cases had been dismissed convalescent, with the exception of four, William Gibson and Mary Maconochie, deaf mutes, both of whom relapsed; Catherine Scott, a delicate girl, who had a very severe attack of fever; and James Milroy, who was detained in the sick ward by inflammation of the epiphysis of the humerus. They are all now convalescent, but the two deaf mutes are still too weak for dismissal.

Having given this short summary of the fever in its medical aspect, I now come to treat of its hygienic relations.

Immediately after the outbreak, an investigation was made with the view of discovering the origin of the malady; the nasal organs of the officials were busily employed in trying to detect noxious exhalations, but for some days they were baffled, till a warden confessed that he often was annoyed by a bad smell in his bedroom, which was in a corridor overlooking the boys' dormitories. This led to a visit being paid to a couple of lavatories, on the same floor with the dormitories, but which, owing to the water-supply during the summer having been very deficient, were not in use. During the day, no bad smell was appreciable, but when they were entered in the morning, an overpowering mephitic odour was emitted from the basins and troughs for washing the feet. These communicated with the drains, but had been trapped in what was considered the best style when the hospital was constructed. The lavatory on the boys' side was much more offensive than that on the girls'.

The boys' lavatory, which contained 14 perforated basins, and a long trough for washing the feet, with a 3-inch waste-pipe, had not been in use from the middle of July, and the girls' one, exactly the same in construction, from about the 1st of August; so the water which trapped the drains must have completely dried up, and the offensive gases made their escape. At the time of the erection of the building, the propriety of having what is called an extraction pipe in connexion with the cesspool was not known. The source of the fever being now accurately ascertained, there was no difficulty in providing for its arrestment; but, unfortunately, the mischief had been done, and 33 cases required treatment before a clean bill of health could be shown. The lavatories were daily flushed, the wards, school-rooms, and corridors subjected to a thorough saturation with repeated fumigation of chlorine, and occasionally with sulphurous acid gas, while extraction pipes were inserted into the cesspools with which the lavatories communicated.

These measures were finally successful in checking the progress of the fever, the period of invasion having lasted for about seven weeks; but as the time occupied by incubation is often of long duration in typhoid fever, I do not believe that the noxious exhalations continued in force after the fumigation and flushing had been resorted to.

Making allowance for what, I presume, will not be disputed, that among so many children some would be more predisposed than others to the fever, it is interesting to observe, that, as a rule, those nearest the sources of contagion were more numerously, and at a more early period, affected, while most of the latest cases occurred among those further removed from the lavatories.

I have constructed a Table, which illustrates these features in the history of the epidemic, and which enables us to see at a glance the date of seizure and of convalescence, and the distance from the foci of infection.

The date of convalescence is not, in many of the cases, the time when the fever had abated, but the period when they were fit to return to the dormitories and class-rooms.

In order to understand the Table, I must premise that the Hospital is constructed in the form of a square, with all the dormitories on the floor above the basement; the hearing boys sleeping on the west side, the hearing girls on the east side, while the deaf mutes occupy the south side.

The lavatories are placed in the centre of the east and west sides.

Most of the dormitories are square rooms, holding twelve beds each, but two or three of them occupying corners are smaller, and only contain six beds.

An analysis of the Table on next page shows that 15, or about one-half of the whole children seized, were hearing boys, and half of these, again, came at an early period of the fever from Nos. 4 and 5 dormitories, which were nearly opposite the most offensive lavatory.

Only 5 hearing girls altogether took the fever, though the dormitories were placed in a corresponding situation to the lavatory.

Though the deaf mutes slept in the front of the building, and, therefore, were far removed from the lavatories, 11 of them were seized with fever, and furnished the worst cases; so that a greater number proportionately were attacked, about 1 in every 6 of the deaf mutes having been ill, while 1 in every 8 was the average of the hearing children.

Another interesting fact to note, is the general prevalence of the exciting cause of the fever, for 19 different dormitories furnished the 31 cases.

The holidays were from the middle of July till the 1st September, but 10 girls and 12 boys remained in the Hospital during the vacation.

TABLE *showing the Cases of Typhoid Fever, the Date of Attack and Convalescence, the Dormitories affected, with their distance from the Lavatories.*

| No. of Cases. | Names of Children. | Deaf Mutes. | | Hearing Children | | Admitted. | Dismissed. | Event. | No. of Dormitory. | Distance from Lavatory. |
|---|---|---|---|---|---|---|---|---|---|---|
| | | B. | G. | B. | G. | | | | | |
| 1 | Janet Cumming, | ... | ... | ... | 1 | Oct. 25 | Dec. 14 | All cured. | 3 H. G. | 14 yds. |
| 2 | Wm. Marshall, | ... | ... | 1 | ... | ,, 27 | ,, 13 | | 6 H. B. | 17 ,, |
| 3 | James Murphy, | ... | ... | 1 | ... | ,, 28 | Jan. 3 | | 3 H. B. | 12 ,, |
| 4 | John Lawrie, | 1 | ... | ... | ... | ,, 30 | Dec 13 | | 1 D. M. B. | 58 ,, |
| 5 | Thomas Russell, | ... | ... | 1 | ... | Nov. 3 | ,, 6 | | 4 H. B. | 4 ,, |
| 6 | Thomas Dott, | ... | ... | 1 | ... | ,, 3 | ,, 6 | | 4 H. B. | 4 ,, |
| 7 | Anne Brown, | ... | ... | ... | 1 | ,, 4 | ,, 14 | | 4 H. G. | 4 ,, |
| 8 | John Glasgow, | ... | ... | 1 | ... | ,, 5 | ,, 6 | | 1 H. B. | 28 ,, |
| 9 | Peter Halliday, | ... | .. | 1 | ... | ,, 5 | ,, 6 | | 4 H. B. | 4 ,, |
| 10 | Robert Wright, | ... | ... | 1 | ... | ,, 9 | ,, 20 | | 5 H. B. | 7 ,, |
| 11 | Rob. Macarthur, | ... | ... | 1 | ... | ,, 9 | ,, 6 | | 5 H. B. | 7 ,, |
| 12 | James Macfie, | ... | ... | 1 | ... | ,, 12 | Jan. 3 | | 4 H. B. | 4 ,, |
| 13 | Alx. Macilreath, | 1 | ... | ... | ... | ,, 18 | ,, 3 | | 1 priv. wd. | 35 ,, |
| 14 | James Milroy, | ... | . | 1 | ... | ,, 21 | Feb. 16 | | 5 H. B. | 7 ,, |
| 15 | John Burns, | 1 | ... | ... | ... | ,, 23 | Jan. 3 | | 1 D. M. B. | 58 ,, |
| 16 | J. S. Donaldson, | ... | ... | 1 | ... | ,, 23 | ,, 3 | | 1 H. B. | 28 ,, |
| 17 | William Gibson, | 1 | ... | ... | ... | Dec. 2 | Still in wd. | | 2 D. M. B. | 48 ,, |
| 18 | William Condie, | 1 | ... | ... | ... | ,, 5 | Jan. 4 | | 3 D. M. B. | 42 ,, |
| 19 | M. Maconochie, | ... | 1 | ... | ... | ,, 5 | Still in wd. | | 2 D. M. G. | 38 ,, |
| 20 | Mary Gourlay, | ... | 1 | ... | ... | ,, 5 | Dec. 20 | | 1 D. M. G. | 46 ,, |
| 21 | J. Macdonald, | ... | ... | 1 | .. | ,, 6 | Jan. 4 | | 2 priv. wd. | 35 ,, |
| 22 | Alex. Ritchie, | 1 | ... | ... | ... | ,, 7 | ,, 3 | | 2 D. M. B. | 48 ,, |
| 23 | Jane Allan, | ... | 1 | ... | ... | ,, 8 | Dec. 29 | | 3 D. M. G. | 36 ,, |
| 24 | David Ross, | ... | ... | 1 | ... | ,, 8 | Jan. 3 | | 3 H. B. | 12 ,, |
| 25 | James Grieve, | ... | ... | 1 | ... | ,, 8 | ,, 3 | | 2 H. B. | 21 ,, |
| 26 | Wm. Draper, | ... | ... | 1 | ... | ,, 8 | ,, 3 | | 1 H. B. | 28 ,, |
| 27 | Catherine Scott, | ... | ... | ... | 1 | ,, 10 | Feb. 15 | | 6 H. G. | 16 ,, |
| 28 | Margaret Cairns, | ... | ... | ... | 1 | ,, 11 | Dec. 20 | | 5 H. G. | 8 ,, |
| 29 | Anne Milne, | ... | 1 | ... | ... | ,, 14 | Jan. 3 | | 3 D. M. G. | 36 ,, |
| 30 | Alex. Ferguson, | 1 | ... | ... | ... | ,, 15 | ,, 3 | | 3 D. M. B. | 42 ,, |
| 31 | Marion Strachan | ... | ... | ... | 1 | ,, 17 | ,, 3 | | 4 H. G. | 4 ,, |
| | Total, | 7 | 4 | 15 | 5 | | | | | |
| | Names of Adults. | | | | | | | | | |
| 32 | E. M., housemd.. | ... | .. | ... | ... | Oct. 25 | Dec. 7 | | Employed in cleaning near the Lavatories. | |
| 33 | J. T., teacher, | .. | ... | ... | ... | Dec. 3 | Jan. 10 | | Bedroom on the floor above, but at a long distance from Lavatories. | |

*Abbreviations.*—H. B., Hearing Boys; H. G., Hearing Girls; D. M. B., Deaf Mute Boys; D. M. G., Deaf Mute Girls.

Of these, 1 girl, Catherine Scott, was seized with the fever, but not till the 10th December, and after several other girls had been taken ill.

Of the 12 boys who continued in the Hospital, 4 took the fever, viz., William Marshall, Peter Halliday, John Glasgow, and David Ross, the first three being among the first attacked by fever.

This would tend to show that longer exposure to injurious influences caused a larger percentage of fever cases. For a third of the boys in Hospital during the vacation were attacked, while, if we except them from the general return, we find that about a sixth only of all the other boys took the disease.

As regards the girls, the numbers are too meagre to be of much value, but the facts point the same way.

1 in 10 of the girls resident during the vacation caught the fever, while 1 in 23 of those who entered the Hospital in September suffered from the disease.

Though it may seem rash to venture on forming any definite opinions from such circumscribed data, I may suggest the following conclusions as derivable from the hygienic facts which were observed:—

*1st.* It took at least eight weeks of exposure to the morbific influences to generate the disease.

*2d.* Those nearest the seat of contagion did not, as a matter of course, suffer first, but rather those who, we may presume, were most susceptible.

*3d.* Those, however, nearest the seat of contagion were in a greater ratio attacked by the disease.

*4th.* Those longest exposed to the foul air were most numerously affected by the fever.

## ARTICLE II.—*On the Production of Presentation of the Face.* By J. MATTHEWS DUNCAN, M.D.

*(Read before the Obstetrical Society of Edinburgh, 13th April.)*

It has always appeared to me to be beyond reasonable doubt, that the immense majority of face cases are the result of derangement of the usual mechanism of the first part of the progress of the fœtal head through the pelvis; or, that presentation of the face is the result of a displacement of the vertex backwards as regards the child, the extension of the head being produced at or near the brim of the pelvis by the propelling powers of labour.

My object in the present paper is to show the mechanism by which this change from a vertex to a face presentation is probably effected.

As has already been hinted at, all cases of face presentation do not demand this kind of explanation. Instances are recorded, for example, where the extreme extension of the head, existing in an ordinary face case during labour, has been observed before labour commenced. To this extraordinary kind of face presentation, and to some others allied to it, I make now no reference whatever, confining myself to the mechanism of the production of face presentation in ordinary circumstances.

Quite recently, a remarkably interesting and valuable contribution has been made to our knowledge of face cases by the pamphlet of Dr Hecker of Munich, entitled "Ueber die Schaedelform bei Gesichtslagen," published at Berlin in 1869. In this work Hecker clearly shows that a great majority of children born with the face presenting have the dolichocephalous form; the height of the skull is small, it is prominent posteriorly, and has narrow and but slightly bulging parietal bones. These conditions of the cranium he demonstrates, so far as his collection of measurements justifies him, to be not the result of the labour, but to be original and permanent states of the heads of children born in this peculiar manner.

This discovery would naturally lead to the supposition that the face presentation is a consequence of the dolichocephalous condition. Hecker does not expressly say that it does, but the direction of his thoughts may be fairly guessed at by regarding the importance which he attaches to the comparative increase of length of the posterior cranial lever-arm, and the nicety with which he has investigated this point. Hecker has omitted to mention another point in the dolichocephalous structure, which, though dependent on the increase of length of the posterior cranial lever-arm, is not identical with it, namely, the projection of the occiput; a condition well indicated by the obtuseness of the mastoid angle or angle formed by the junction of the squamous and lambdoidal sutures.

The production of face cases, as I am about to describe it, does not depend upon the increased comparative length of the posterior cranial lever-arm, nor on the projection of the occiput, but depends upon a quite different mechanism. This I shall presently describe. But I wish here to point out the place and power of the dolichocephalous form, as introducing a condition in which the mechanism alluded to will operate at extraordinary advantage, and therefore will operate with extraordinary likelihood of producing the transformation. The transforming mechanism will produce the transformation with greater certainty and ease if the child is dolichocephalous than if its head is of ordinary construction.

The mere dolichocephalous condition explains nothing, for even in the dolichocephalous the comparatively long posterior cranial lever-arm is shorter than the anterior cranial lever-arm.[1]

In ordinary labour, with the vertex presenting, whether the occiput looks to the mother's right or to the mother's left, transformation into a face case is prevented by the greater length of the anterior cranial lever-arm. Strong pressure equably exerted on the upper surface of the cranium, as it will be if the longitudinal axis of the uterus is parallel or nearly so to the axis of the pelvic brim, will lead to flexion, not extension of the fœtal head. In other words, if there be uniform resistance to the advance of all parts of the cranium, as there will be in the condition of the uterus above named, the occiput will advance first, if the resistance is sufficient to produce flexion of the head. If, in the same circumstances, the posterior cranial lever-arm were the longer, then extension of the head and forehead presentation would result, the first step towards face presentation, which would subsequently be produced.

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[1] In Hecker's eight cases of delivery under face presentation, in which he made careful measurements of the anterior and posterior cranial lever-arms, the posterior was shorter in every case than the anterior. The average difference between the two was, in fœtal heads generally, 1 centimetre. In the eight face case heads, it was ·4, ·1, ·2, ·4, ·6, 1, 1, 1·7.—*Ueber dis Schaedelform, etc.*, s. 46.

The above statements may be held as true, because the relative length of the cranial lever-arms is the only disturbing element of what would otherwise secure uniform progress in unchanged conditions. In that part of the pelvis where transformation of the vertex into a face case takes place, the axis of the passages is one and unbent, except by the usual slight lateral obliquity of the uterus to the right. To the bending or curvature of the passage produced by this right lateral uterine obliquity we shall afterwards draw particular attention. The curvature of the genital passage which begins posteriorly about the middle of the third sacral vertebra, a curvature in the antero-posterior vertical plane, is an important curvature in the mechanism of parturition; but it is not important in the subject now under discussion, for the simple reason that the transformation of a vertex into a face case is ensured before the cranium affronts the commencement of this antero-posterior curvature in the vertical plane. Were this great curvature not so situated, its influence would be great. It is very great in the mechanism of the descent of the head through it under a pressure with a uniform direction. But this has no relation to presentation of the face, which, no one can doubt, is determined before this part of the genital passage is reached.

Although the great antero-posterior vertical curvature of the genital passage does not interfere in the production of face cases, there may be interference from another curvature, which is produced by the well-known usual right lateral obliquity of the uterus. This very frequent obliquity produces a curve of the genital passage at or near the brim, whose mesial line is concave to the right. No doubt, the so-called erection of the uterus during pains will tend to diminish, and, perhaps, will at last obliterate, this curve, bringing the uterine axis into the antero-posterior mesial vertical plane. But, while it is not proved that this complete obliteration of the right lateral obliquity of the uterus takes place at the acme of a so-called uterine erection during a pain, it is certain that, at the beginning and ending of a pain, there is, in the sitting or standing position, or in the dorsal decubitus, a right lateral obliquity of the uterus, and consequent curvature of the genital passage, whose influence in producing the transformation of a vertex into a face case must be carefully considered.

To show how frequent is the deflection of the uterus to the right, I shall quote some figures from the work of MM. Dubois and Pajot referring to this point:—"Of 100 pregnant women," say they, "taken without selection, and examined at the Hospice de la Maternité, at the commencement of the ninth month of pregnancy, 76 presented a very evident right lateral obliquity; 4 a left lateral obliquity; 20 an anterior obliquity without appreciable lateral inclination."[1]

Now, in pushing a plastic body, such as a fœtus, through a curved tube, such as the pelvis, and supposing that the resistance is equal at all parts of the tube, that part will be pushed with greatest advantage which is nearest to the line of the propelling force, and it will advance first. In the example referred to, the parts nearest the concave side of the passage will advance first.

We now proceed to apply this general statement to the subject under discussion.

In the great majority of cases of right lateral obliquity of the uterus, the occiput of the fœtus occupies the concave side of the passage of the genital canal at or near the pelvic brim. If the head meets with much resistance, the occiput will tend to advance first, from the comparative shortness of the posterior cranial lever-arm, and from its position nearer the concavity of the passage, and therefore nearer the line of the propelling force, which must incline towards the left in consequence of the deflection of the uterus. The vertex presentation will be maintained.

In a certain proportion of cases, nearly 1 in 3½, the fœtal occiput looks to the right side of the passage, to the convexity of the canal, and the forehead to the left side or concavity of the canal. The propelling power, *per se*, will therefore tend to make the forehead descend, and bring on a forehead presentation in anticipation of a fully-formed face case. This tendency to push down the forehead instead of the occiput will be, in the majority of cases, counteracted by the agency of the more efficient resistance by the soft parts to its advance in consequence of the greater comparative length of the anterior cranial lever-arm. But, in a case of exaggerated right lateral obliquity, this resistance may be overcome, and the forehead may descend. In every case in which the dolichocephalous form exists, the resistance from the comparative length of the anterior cranial lever-arm will be diminished, or, in some cases, even almost annihilated, and the forehead be invited to descend first, the head becoming extended.

Now, it cannot fail to strike the intelligent student, as being more than a coincidence, that while right lateral uterine obliquity is the ordinary one, face cases are comparatively much more frequent in the second position—that is, with the occiput looking to the right or the convexity of the canal, before the transformation; and that the children are generally dolichocephalous. In other words, the conditions which can be shown, *a priori*, to be likely to lead to the transformation of a vertex into a face case are exactly the conditions which are found, not indeed invariably, but to prevail.

"The numbers," says Professor Hecker, "which I have previously given for the proportion of the first to the second face position, 22 : 21, are now changed, so that now 28 first and 21 second positions are noted; the proportion of the latter to the former, therefore, is expressed as 1 : 1·3, and if I compare with this my former calculations in reference to first and second vertex positions, which yielded a proportion of 1 : 2·3, then I must justify Winckel when he judges from his statistics, 'that the second position of the face in proportion to the first occurs oftener than the second position of the cranium to the first; that, therefore, *ceteris paribus*, a face position is easier produced when the back of the child lies to the right than when it lies to the left.' "—(S. 15.)

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[1] Traité Complet de l'Art des Accouchements, tome i. livr. 2me, p. 381.

It appears to me, then, to be highly probable that the chief cause of face cases is obliquity of the uterus in any direction, ensuring a curvature of the genital canal at the brim of the pelvis; that this cause operates when the forehead of the child is placed near the concavity of the curved canal, or nearest the line of the propelling force; and that the dolichocephalous form will greatly favour the transformation under these circumstances of a vertex into a face case.

Before the study of face cases led me to the above-mentioned results, I, and others, used to think and speak of hitching or obstruction of the occiput on some part of the brim of the pelvis, leading to its complete arrestment, and the subsequent extension of the head, the forehead, and then the face descending. This hitching, if it ever takes place, will be evidently favoured by the projection of the occiput in the dolichocephalous form. But such ideas were admittedly very vague and unsatisfactory, for such hitching can scarcely even be conceived except in a case of deformed pelvis, and it is only a small proportion of face cases that actually occurs in complication of labour with deformity of the brim of the pelvis. Though the hitching of the occiput is a very rude and utterly insufficient explanation of the majority of cases, it may possibly occur in some of the face cases which are produced where there is deformity of the brim. I am far from saying that face presentation is so produced in cases of pelvic deformity, for examples of this are far too little known; their peculiarities have been as yet far too little studied to enable us to come to any conclusion whatever on the subject of their special causation.

Before concluding, I shall draw attention to a very remarkable circumstance, which the above proposed theory of face cases may explain. I find that Hecker and other foreign authors ascribe to face cases a frequency far in excess of what is, I believe, observed in this country. Hecker says there is a face case in every 168. In Collins's Dublin Hospital experience, there was only 1 in 497. Now, this remarkable difference[1] may be explained by differences in the position of the women during labour in various countries. The almost invariable decubitus of British women on the left side may prevent the transformation of many vertex into face cases that would otherwise be so changed. I am not sufficiently informed to contrast this position of British women with the dorsal decubitus or other position of German women in like circumstances. But it appears to me to be a subject worthy of attention.

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[1] "Thus," says Churchill (Theory and Practice of Midwifery, 4th ed., p. 409), "in British practice, out of 141,259 cases, there were 567 face presentations, or 1 in 249; among the French, 50,141 cases, and 189 face presentations, or about 1 in 265¼; and among the Germans, 69,417 cases, and 411 face presentations, or about 1 in 169¼; the whole giving 1167 face presentations in 260,817 cases, or about 1 in 223½ cases."

## ARTICLE III.—*On a New Splint for the Knee-Joint.* By THOMAS ANNANDALE, F.R.S.E., Surgeon to the Edinburgh Royal Infirmary.

HAVING found the splint figured and described below of great service in the treatment of cases of excision of the knee-joint, and believing also that it may be usefully employed in many affections and injuries of the same joint, I venture to recommend it to the notice of my professional brethren. The splint, which is made principally of tin, consists, like the Macintyre and Liston splints, of foot, leg, and thigh pieces, but its hollow is much deeper than that of the latter, and so permits a limb placed in it to lie more securely. A most important advantage in this splint is, that it can be readily shortened and lengthened, both at the junction of the leg and thigh pieces, and also at the junction of the foot and leg pieces. In addition, the splint is light, clean, cheap, and easily swung in a Salter's or other cradle. The first splint which I had constructed only admitted of being adjusted in the straight position, but being anxious to make the splint capable of flexion and extension, I sought Mr Hilliard's aid, and he has adapted a most ingenious modification, illustrated in Fig. 3, which successfully accomplishes these movements.

The foot-piece (Fig. 1, *a*) consists of a sole made of wood, and two iron rods, which are connected to either side, and pass up to join the leg-piece. The leg-piece (Fig. 1, *b*) is made of tin, bent into a hollow so as to receive the leg; the edges of the tin are turned over on each side so as to make a tube, into which the iron rods of the foot and thigh pieces are inserted. In addition, the leg-piece is furnished with two screws, *f*, *f*, for fixing the rods, and also with a hook on each side for swinging the splint. The thigh-piece (Fig. 1, *c*) is, like the leg-piece, composed of a hollow of tin, with each lateral edge turned over, and made into a tube, in which the upper ends of the rods connected with this part of the splint are fixed; the iron rods are bent out so as to form a curve,[1] which allows any wound over or near the joint to be readily dressed; the lower ends of these rods pass down to join the leg-piece. Two hooks, *g*, *g*, one on each side, are also connected to this portion of the splint, and assist in swinging it. In adjusting the splint, the free ends of the rods connected to the sides of the foot and thigh pieces are

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[1] If the splint is used in a case where there is no wound or sore, these rods need not be curved, but may be made straight.

FIG. 1.

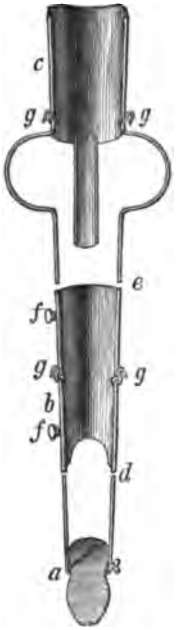

FIG. 2.

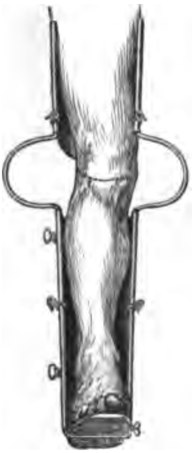

**Fig. 1 illustrates the three portions of the splint separated.**

*a*, *b*, *c*, The foot, leg, and thigh pieces.
*d*, The free extremity of the rod connected with the foot-piece on one side, which slips into the lower end of the tube at the edge of the leg-piece.
*e*, The free extremity of the rod connected with the thigh-piece, which slips into the upper end of the same tube.
*f*, *f*, The screws for fixing the rods of the foot and thigh pieces when they have been inserted to the required length.

**Fig. 2 shows the limb, in a case of excision of the knee-joint, placed in the adjusted splint.**

slipt into the corresponding tubes which run along the upper edge of the leg-piece. When the splint has been thus adjusted to the required length, as shown in Fig. 2, its various pieces are fixed by turning the screws, *f*, *f*, the ends of which pass into the tubes, and compress the rods. Before placing the limb in the splint, the latter requires to be well padded, and for this purpose I use two flat pillows covered with thin sheet gutta-percha; one pillow being made to fit the thigh-piece, and the other the leg-piece. When the limb has been accurately laid in the padded splint, it is secured in position by means of one or more bandages, as in the case of the Macintyre splint. The apparatus and limb can then be swung in a cradle, which allows the patient to lie more comfortably and move in bed much more freely than if the limb had been unsuspended. The modification of this splint, as shown in Fig. 3, is made of the same materials, but the thigh and leg pieces, instead of being connected by the iron rods, are fixed together by means of a hinge, *a*, and four narrow plates, *b b*, *b b*, which are separated or brought together by means of a right and left hand screw, *c*, and thus produce flexion or extension of the apparatus opposite the knee.

FIG. 3.

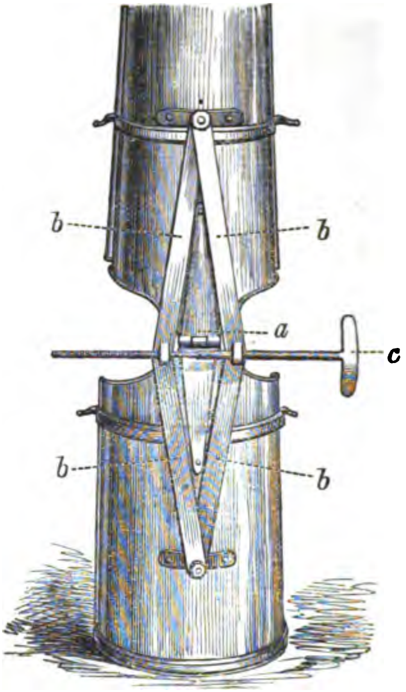

**Fig. 3 represents the posterior aspect of the splint, with Mr Hilliard's modification for flexion and extensiou.**

***a*, The hinge connecting the thigh and leg pieces.**
***b b*, *b b*, The four narrow plates which flex and extend the leg and thigh pieces when separated or brought together by turning the screw *c*.**

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## ARTICLE IV.—*A Peculiar Paralytic Condition of the Lower Extremities following Gastric Fever.* By BENJAMIN BELL, F.R.C.S.E.

A BOY in George Watson's Hospital, about twelve years old, of much brightness and vivacity, and uniformly healthy, was taken ill, on the 7th of October 1869, with symptoms of fever. There was no salient local affection to account for the constitutional disturbance; but gradually, gastric irritation manifested itself, by irritability of the stomach, by tenderness on pressure, by looseness of the bowels, and rawness of the tongue and fauces. While this condition lasted, the patient complained a good deal of pains in his lower extremities, and, for some days, there was more or less muttering delirium and mental confusion.

The treatment need not be described. It was mainly dietetic, with poultices to the abdomen; and latterly the infusion of logwood seemed to be very efficacious in arresting the diarrhœa, which had proved obstinate.

As convalescence advanced, it became apparent that he had lost entirely the power of locomotion, and even of maintaining the erect posture. That this was different from mere debility, was rendered obvious by the circumstance, that he was able, while in the recumbent posture, to raise either of his limbs from the horizontal position, and to point the toes in any direction with accuracy and precision. It may be noted, that the upper extremities were not implicated at any period of the affection, and that the tongue and muscles of the face preserved the same immunity.

When taken out of bed, and firmly supported on both sides, he had no regulating power over his lower extremities. They moved at his bidding, but in an erratic, jerking manner, with a continual tendency to diverge, reminding one of the spasmodic movements in chorea, with this difference, that his limbs had no tendency to move without an exertion of the will.

Sensation was normal, and no tenderness along the spine could be detected.

For nearly a month after these paralytic symptoms were discovered, no improvement took place, although his general health was excellent, and all the functions were performed in a satisfactory manner.

The treatment now consisted of iron and nux vomica, with friction of the spine and lower extremities.

Becoming anxious as to the result, although still hopeful of ultimate recovery, from the similarity of the phenomena in some respects to what we meet with in cases of diphtheria, I availed myself of a consultation with Professor Sanders. In accordance with his advice, the iron and nux vomica were given up, and in their room arsenic and belladonna substituted; two drops of Fowler's solution, and one-sixth of a grain of the extract, being administered three times a day in a bitter infusion. This treatment began on the 24th of November, and on the 16th of December, the following note was made: "During the last fortnight, a gradually increasing power of setting down the feet and regulating the limbs has returned." It is right to mention, however, that about the same date I found it necessary, or at least desirable, to discontinue the medicines, as the tongue was getting furred, along with dilatation of the pupil, and considerable disturbance of vision. But the improvement went steadily on notwithstanding; and before the end of December the patient had regained completely his power of locomotion.

I do not remember to have met with a similar case.

Dr Graves of Dublin, in his "System of Clinical Medicine" (1843, p. 401), relates two cases, resembling in essential points the one now detailed. The first was a lad of fourteen, who had disordered his stomach and bowels by eating a large quantity of nuts. After a very serious illness and active treatment, he was allowed to leave his bed. On attempting to do so, it was found that he was paralytic in his lower extremities. He had perfect power over his arms and trunk; the paralysis was entirely confined to the muscles; sensation was perfect. The second case was a lady affected with gastro-duodenitis, terminating in an affection of the liver. After an alarming illness, and a course of severe remedies, she became convalescent, but in spite of her returning health, she too was found to be paralytic in her lower extremities.

Dr Graves says:—"In neither of these cases was the loss of muscular power so great as to deprive the patients of the use of their legs while lying in bed. They could then be raised, flexed, and extended with apparent ease and strength; and yet, when the patient attempted to stand up or walk, he was totally unable to do either, his legs sinking under him; and even when supported by a person on each side, so as to take the greater part of the weight of the body off the limbs, he was still unable to advance one foot before another. I cannot understand why so great a difference should exist between the muscular force of the legs in the one position and in the other."

The boy, it is right to mention, recovered his power of locomotion completely; the lady remained paralytic.

Dr Graves endeavours to explain these phenomena by referring to the anatomical connexion betwixt the nerves of the great sympathetic and the branches of the spinal nerves, and by supposing that morbid impressions made on the extremities of the former may be rapidly and extensively propagated to the spinal cord, and thence, by a reflex action, to the muscular nerves of the lower extremities.

This explanation, however, is obviously defective, for this reason, that while abdominal affections analagous to those occurring in his two cases are very common, the peculiar form of paralysis which he so well describes is extremely rare.

Without attempting to dogmatize or speculate on a subject surrounded with difficulties, I content myself with giving an accurate record of the phenomena, and with drawing attention to the similarity which they bear in some respects—with obvious differences, however—to the form of paralysis which often follows diphtheria.

## ARTICLE V.—*On Rapid Pulmonary Congestion, and Pulmonary Apoplexy, as a Cause of Death after Surgical Operations or Injuries.* By JOSEPH BELL, M.D., F.R.C.S., etc.

SURGEONS are sufficiently familiar with the effects of pyæmia, septicæmia, and hectic fever, as evinced in inflammation, sometimes sufficiently acute, of internal organs, and especially of the lungs.

Pneumonia and pleurisy may be the result of direct injury, as from a fractured rib; spread of inflammation, as after the operation for ligature of the subclavian; systemic infection, as after some operation or injury involving suppuration.

Metastatic abscesses are too familiar in pyæmia. Tubercular deposits may precede, accompany, or result from the exhaustion to the system produced by a scrofulous joint.

Pulmonary apoplexy, in which rupture of the pulmonary tissue has taken place, or acute pulmonary congestion, stopping short of apoplexy, are not so frequent, or at least are not so much spoken of, as causes of rapid death after surgical disease or injury.

In cases of fracture of the cervical vertebræ, nearly all surgeons are agreed, that if the fracture involve the cord, at or above the level of the fourth vertebra, the patient will die speedily from interference with his respiration, from having only the diaphragm to trust to; and even if the fracture be below the origin of the external thoracic nerve, still the diaphragm, even aided by the serratus magnus, is unable to carry on respiration with sufficient power to prevent pulmonary congestion and accumulation of mucus in the bronchi. It is remarkable how very little exact information regarding the pathological condition of the lung, in such cases, we find in the more common English text-books.

I must say that, to my mind, passive congestion and accumulation of mucus in the bronchi are both quite insufficient to explain the rapidity of death, and the severity of pulmonary changes, in many cases of spinal injury.

For example, the following:—

CASE I.—W. W., æt. 22, a very strongly built healthy young man, was sleeping on some sacks, when a friend (?) threw down another sack, weighing about 12 stone, on his head, dislocating the third and fourth cervical vertebræ from each other, and fracturing the body and left articular facet of fourth vertebra. This was at 6 A.M. He was at once paralyzed from the neck downwards, with the exception that he could slightly raise the right arm. His breathing was purely diaphragmatic from the first. He was torpid and slightly livid, and complained chiefly of pain in his neck.

At 12 A.M., lividity and torpor had increased; his extremities were cold, and his breathing embarrassed. He died exhausted at 3 A.M., or just twenty-one hours after the accident.

Both lungs were much engorged with venous blood, and the right contained in its posterior lobe a large pulmonary apoplexy, of the diffuse form, occupying the greater part of the lobe behind. The heart and liver were both congested, and also soft and flabby in texture.

Still rarer and more inexplicable is the result of a case like the following:—

CASE II.—Mr W. D., æt. 46, fell off his horse, which probably trampled on his chest. He had lain for some minutes on the ground in an insensible condition, and was picked up and brought into town (four miles) in a carriage.

I found him in bed, still very drunk, but not in the least comatose. He had two severe scalp wounds, one over right temple, the other over occiput, each about two inches in length, neither penetrating to the bone, but only exposing the periosteum. The temple wound had bled most profusely, but the bleeding had been stopped by Dr Pattison, who was in attendance. He had two broken ribs, the sixth and seventh on the left side, close by the nipple. He was bruised in many places, and had an ugly lacerated wound of left thigh several inches long. Various wounds dressed. He was very restless, vomited frequently dark bile smelling strongly of whisky. Pulse weak, 100.

22*d*.—Next morning he was much better, still not quite himself; after a restless night, considerable pain in chest, still no pulmonary symptoms whatever, though carefully examined.

*Vesp*.—About six this evening, after a good day, he suddenly was seized with a fit, described as a convulsion fit, after which the most sudden and frightful breathlessness, with extreme lividity, pale lips, tossing to and fro, and throwing his arms about, fighting for breath; yet nothing in the larynx, trachea, or bronchi; no obstacle to the *admission* of air. Pulse 120, weak and fluttering. The symptoms were those of hæmorrhage into one or other pleura or into the pericardium, yet all the physical signs were absent. The dulness was equal in both bases; there was no bulging of the rib spaces, and no churning noise with the heart-beats. There were râles over the whole chest, large and loud, obscuring the finer breath-sounds.

He was ordered sinapisms, hot foot-baths, hot cloths to his arms, whisky, ether, etc., and his relatives were telegraphed for at once.

23*d*.—After a fearful night of struggle for breath, he was still alive at 8 A.M., contrary to expectation, but much weaker, and he sank at 12 noon.

*Post-mortem* examination of chest and abdomen only. The body, an exceedingly powerful well-nourished man. The fractured ribs were found as diagnosed, with slight laceration of lung at fracture. Both lungs were absolutely solidified with pulmonary apoplexy, hard, black, and heavy, with scarcely any breathing space left. Bronchi were full of a profuse muco-frothy discharge. The heart was flabby and very ill contracted; the right side contained a large loose clot. The liver was friable, yellow, apparently in the early stage of fatty liver. The other organs were apparently absolutely healthy.

None of the circumstances of the accident, nor surgical lesions, sufficed to explain the pulmonary changes and the rapidity of the fatal issue. It may be said that two ribs were broken, and broken inwards, causing laceration. True; and doubtless, had there been time, pleurisy might have resulted, and even a local pneumonia centring from the injured portion of lung. But the right lung was even more destroyed than the left, and the apoplexy in both was central, not peripheral.

Neither can we explain it by any injury to the cord. There was not the slightest evidence of anything of the kind; the patient's muscular power was unimpaired almost to the end. He made no complaint of injury to the neck, nor was there any bruising. Exposure to cold will hardly explain it. The patient was a man of most robust health and Spartan habits, invariably writing under an open window even in the depth of winter. The weather was not very cold, and he had not lain long on the ground before being picked up.

On discussing this question with my friend Dr George Balfour, he most kindly directed my attention to a very interesting clinical lecture on this very subject, lately delivered by Professor Verneuil of Paris,[1] which, without shedding much light on the causation, gives a vivid description of the lesion, and details some illustrative cases. I quote from it:—

"In enumerating certain causes of unforeseen and sudden death in surgical cases, I have omitted a lesion which, from its gravity and suddenness, ought to have appeared in our list. I mean pulmonary congestion, frequently accompanied by pulmonary apoplexy or rupture of the tissue of the lung. I have five times met with this formidable complication. I have recognised it only in the post-mortem theatre, when seeking the explanation of sudden death, and have never yet diagnosed it during life. I hope that now, when my attention is aroused to the subject, I may in future diagnose it when it occurs, and I believe that often it is not recognised. My observations were entirely in surgical wards, and all my cases suffered from some surgical disease which acted as a predisposing cause. These were very various: erysipelas, glandular inflammation, septicæmia, delirium tremens, and hernia. All the patients were males, two young, aged 24 and 25; two old, 60 and 72; the last 46 years old. In all the lesion was identical. Both lungs attacked at once and in every part, containing little air, resembling in colour a blood-clot, and engorged with thick, black, viscid blood.

[1] Gazette Hebdom., Oct. 22, 1869.

"Except the congestion and apoplexy, evidently quite recent, the lungs showed trace of no old lesion whatever, neither tubercle, pneumonia, or pleurisy. The heart presented no valvular lesion.

"The blood was always very black, thick, and viscid; indeed, exactly such as one meets with in severe fevers. Death was always very rapid, preceded by an agony of some hours, and certainly caused by the pulmonary disease, before the appearance of which the patients were indeed seriously ill, but still in no apparent risk of death, which in each case took us by surprise. Once only was bloody sputum noticed, twice vomiting existed, never any cough to speak of. So far as the lung went, the symptoms were reduced to a minimum."

I can here only very briefly abstract M. Verneuil's cases:—

CASE I.—Man, aged 24, entered hospital for an acute conjunctivitis of one eye, for the relief of which ten leeches were applied. Next day symptoms of erysipelas showed themselves, for which he got a purge, and died so suddenly in the night, that nothing was observed.

*Post-mortem.*—Absolute health of every organ except the lungs, which were the seat of intense congestion. The lower lobe of the right lung was so thoroughly apoplectic as to be a bag of blood; the corresponding pleural cavity was also full of black half-coagulated blood.

CASE II.—Man, æt. 25, strong and perfectly healthy, got his elbow bruised between two buffers. The humerus was broken, and on the twelfth day after the accident the broken fragments were removed, and eventually the joint excised; after which, the next day, he died rapidly after delirium and restlessness.

*Post-mortem.*—No phlebitis, no angioleucitis, no pyæmia; the lungs were completely hepatized, but there were no special apoplectic portions.

CASE III.—B., æt. 72, after a superficial injury to his leg, suffered from inflammation of the lymphatics, was feverish for two or three weeks with delirium, and considerable albuminuria. He began to improve in general health, but had a series of small abscesses along the inflamed lymphatics. He then lost his appetite, had frequent and uncontrollable vomiting, and died very suddenly without any lung symptoms being noticed.

*Post-mortem.*—Some interesting maladies of the gastric arteries and of the intestines were observed, which I need not detail.

Both lungs throughout their whole extent resembled in colour the pulp of the black cherry called guigne (*gean*). They contained little air, but a large quantity of thick fluid blood. The right lung was in a condition of true pulmonary apoplexy, but contained several points of ill-defined metastatic abscess.

CASE IV.—An emaciated man of 60 had a very large hernia incarcerated for eighteen days, but not actually strangulated. He was ordered rest, purgatives, and injections; no attempt at reduction was made. He was also ordered a bath. He had scarcely been a quarter of an hour in the bath when his breathing became embarrassed, his extremities livid. He was removed to his bed, and, notwithstanding all treatment, died in an hour or two.

*Post-mortem.*—A large hernia, with no constriction, adhesion, or inflammation except in one part, where there was considerable ecchymosis. Liver was fatty. The lungs were immensely congested, and here and there the left lung showed patches of apoplexy.

CASE V.—A cabman suffered a simple fracture of his right fibula by falling off his cab when drunk. On the third day after admission he became delirious, knocked his limb about, and next day died very suddenly with embarrassed respiration.

*Post-mortem.* — Organs congested, but apparently otherwise healthy. Lungs were both congested in the most extraordinary manner, containing very little air, but on section discharging quantities of black blood.

As to the *causation* of this fortunately rare complication of surgical lesion, it is easy to theorize, but more difficult to prove the correctness of the theory.

I would be inclined to seek the connexion between the surgical lesion and its pulmonary complication in the *nervous system*, and probably in that little known system the *vasomotor* or sympathetic. Some disturbance to the branches regulating the tension of the pulmonary vessels might easily explain the rapidity and extent of the disturbance to the physical phenomena of the pulmonary circulation. We find this disturbance in *purely medical disease*, such as fevers, diphtheria, and the like, and we are not unprepared for internal visceral congestion in them.

We know that in certain stages of the progress of a *burn*, visceral congestion may be expected, partly from the increased work given to the viscera by the skin being so far thrown out of working order, but partly, also, as a result of shock to the nervous system.

We need not be surprised at similar phenomena occasionally following surgical injuries. From Bernard's experiments we know that in animals injuries to the cervical ganglia of the sympathetic are rapidly followed by pulmonary symptoms; chiefly, however, rapid effusions into the serous membranes, with extensive inflammation, and rapid formation of false membranes.

*The pneumogastric* may possibly be to blame. My second case, and Verneuil's third, were both accompanied by severe gastric symptoms, especially vomiting; yet Verneuil's gave no reason to suppose that any injury of the pneumogastric could have taken place. In mine it might have been injured by the wounds at the occiput. The experiments of Legallois, Claude Bernard, and others on animals, show that, in some cases after section of the pneumogastric, the lungs very rapidly pass into a condition of hepatization or extremely rapid congestion; while in others this symptom is absent, and the stomach seems to be the organ chiefly affected.

My object in bringing these few remarks on this interesting but obscure subject will be attained, if I can elicit the opinions of some of my medical brethren, and obtain from them information both on its pathology and treatment.

## ARTICLE VI.—*Extirpation of a Kidney in Man.* Preliminary Communication by PROFESSOR G. SIMON (of Heidelberg), 10th April 1870.

[We are glad to be able to place before our readers the first authentic account which has appeared in Britain of the highly interesting case of nephrotomy, by Professor Simon of Heidelberg. It is a translation from an article by that eminent surgeon in the *Deutsche Klinik* of last month, and has been communicated by the Baron Paul von Seydewitz, M.D., etc.]

IN some English and French journals I find communications regarding the extirpation of a human kidney which I performed in August last at the surgical clinique of Heidelberg. These communications are due to medical men who, on their journey through Heidelberg, have seen and examined the patient. As, however, the said communications contain several inexact and erroneous statements, and as the publication of a full description of the case may most likely not take place for some time, I may as well now give the following short abstract. I feel the more justified in so doing, as a sufficiently long space of time has elapsed since the operation, to enable us to judge fully of its results.

Our patient (a labourer's wife, æt. 46) was operated upon by Dr Walther of Offenbach, on account of a cystoid tumour of the ovary, one year and a half previous to her admission into the Heidelberg Surgical Clinique.

After the abdominal incision was made, it was discovered that the ovarian tumour was so intimately connected with the very enlarged uterus, that that organ had to be removed at the same time as the degenerated ovary—*i.e.*, that ovariotomy had to be combined with hysterotomy. But the ovarian tumour was not alone connected with the uterus, but also with the left ureter; so that, at the removal, the ureter was severed in its whole circumference.

The patient recovered, but an abdominal-uretral fistula remained, through which all the urine which was produced by the left kidney involuntarily escaped. I attempted to cure this intolerable state by trying to make a communication between the ureter and the bladder, and by a subsequent occlusion of the abnormal passage, which opened through the abdominal walls and into the vagina. But after many unsuccessful attempts, during which even the life of the patient was several times at stake, we had ultimately to give up this plan of cure. Attempts to produce artificial occlusion of the ureter (and by that means obliteration of the kidney) had also to be abandoned on account of very dangerous symptoms, which made a favourable result most doubtful.

Ultimately I contemplated extirpation of the kidney. By perusing the literature of the day, by experimenting on dogs, by anatomical researches, and by comparing this operation with other somewhat similar operations which had been introduced into surgery, I had convinced myself that, in our case, nephrotomy was not only justified but even indicated. Consequently, I performed extra-peritoneal nephrotomy in presence of a great number of medical practitioners and students, after having stated the reasons which, in my opinion, urged me to perform the operation. The patient stood the operation pretty well, and, after six weeks, was so far advanced towards recovery that she could leave her bed. The ligatures of the pedicle did not show any sign of detachment, so I did not try to remove them forcibly, because there was increased suppuration and pain whenever strong traction was made. After six months the ligatures came away with comparatively slight traction. Two days afterwards, the sinus in which they were embedded was closed, and thus the whole wound was cicatrized.

After the ovario-hysterotomy there remained a contraction of the muscles of the calf of the right leg, which took a long time to cure. The patient, whose health, as may well be imagined, had been seriously impaired in consequence of all the operations which she had undergone within three years, is now in a most satisfactory state of health. She is engaged all day in needlework, and sometimes takes long walks in the environs of Heidelberg. The reason that she has not been long ago discharged is, that we wished to have her as long as possible under observation, and because we knew that she must, on going home, return to very reduced circumstances.

These are the chief points of our operation, which hitherto has not been attempted in man. In a pamphlet on the case, which will be published in a couple of months, I shall enlarge on the admissibility of nephrotomy in my case; then I shall give the history of the case, and describe the operation, and shall discuss the bearing of my case on the operative treatment of some diseases of the kidney; concluding with observations at the bedside, and the relation of the experiments on animals, which I have deemed necessary for the decision of some physiological and pathological questions no less interesting than important.

## ARTICLE VII.—*On Two Cases of Diphtheritic Paralysis simulating Locomotor Ataxia.* By THOMAS GRAINGER STEWART, M.D., F.R.C.P., Physician to the Royal Infirmary.

IN the standard works treating of Locomotor Ataxia, Muscular Anæsthesia, and Diphtheritic Paralysis, I find no distinct reference to the occasional occurrence of cases in which there is some difficulty in establishing a diagnosis between the last-named and one of the first two affections. Cases certainly do occur in practice in which such a difficulty exists; and as good examples, I propose to bring under the notice of the Society the history of two patients who have lately been under my care.

I shall take first the case which occurred more recently, as it was better marked, and more accurately recorded.

On the 4th of December last, John Roxburgh, an engine-stoker, married, and 32 years of age, was admitted to Ward VII. of the Royal Infirmary, complaining of unsteadiness of gait, sore throat, and unfitness for work.

His illness, which had commenced about the middle of September, he ascribed to cold, which he caught on coming into the air after working at the valve of the tender of his engine at a very high temperature. He had formerly enjoyed excellent health, and had never met with any accident. He was a temperate man, of regular habits, and lived in a healthy part of Berwick-on-Tweed. His illness commenced with shiverings, which immediately followed his exposure to cold. A day or two after, he suffered from sore throat, and had to discontinue work. He was attended by a medical man who recommended the application of mustard to the neck, applied caustic to the throat, and gave him some medicine. He had a good deal of pain in the throat, some difficulty in swallowing, and noticed that occasionally, when attempting to swallow fluids, he was seized with what he considered spasms in the throat, and spluttered the fluid from his mouth and nose. He recovered from this attack (of sore throat) in a fortnight, and, though feeling weak, was able to resume his work, and continue at it for three weeks. Being closely examined as to his condition during these weeks, he states that for a couple of days he had a dropping of the right eyelid, that his companions said he squinted, and that when he had occasion to look at the railway signals, he had to cover his right eye. He felt unsteady in his gait, and when he washed his face he fell forward, unless he supported himself with one hand. He also had a severe pain, which he referred to the left hip-joint, but never had sharp shooting neuralgic pains, a sensation of grasping round the waist or elsewhere, nor of numbness or formication, excepting that for two days in the beginning of his illness he had the last named sensation over some of the dorsal vertebræ. There was no incontinence of urine nor seminal emission. Towards the end of the three weeks, his weakness increased, and his throat symptoms recurred, compelling him to leave his employment. During the few weeks which elapsed between this time and his admission into the Infirmary, no further symptoms occurred, excepting that his voice lost its natural quality for some time, and became treble, like that of a woman.

At the time of his admission, his condition, as recorded by Mr Evans, one of the clinical clerks of Ward VII., was the following:—

*Family History.*—Good.

*General Appearance.*—The patient is a tall, healthy-looking man, of sanguine temperament, and ruddy complexion.

*Alimentary System.*—Teeth and gums natural; tongue furred and moist; secretions of the mouth increased. There is slight ulceration of the tonsils, most marked on the left side; the uvula is œdematous; the parts are not congested. He is unable to swallow solids. On the day of his admission, he could not swallow water. Upon attempting to do so, he was seized with spasms in the throat, which caused it to be spluttered through the mouth and nose, as it had been at the beginning of his illness. Appetite good, and alimentary system otherwise normal.

*Circulatory, Respiratory, Integumentary, and Urinary Systems.*—Normal.

*Nervous System.*—Sensibility to touch, heat, and pain, is normal, excepting that it appears diminished in the soles of the feet, for when the patient walks, he feels the ground soft under him like feathers or cotton. The special senses are normal. He walks with a staggering gait, has difficulty in turning round, and if he walks quickly, feels as if about to fall. On shutting his eyes, the difficulty in walking is much increased. On standing with the legs apart, he feels tolerably steady; but on shutting his eyes, and placing his feet together, he begins to stagger, and would fall if not supported. He grasps objects firmly with both hands, can seize and hold small objects accurately. When lying in bed he can move his legs and feet, and, so far as by careful investigation can be ascertained, he possesses full muscular power. There is no spinal tenderness. Memory and attention normal, and he sleeps well.

From these symptoms I was inclined to regard the case as a somewhat anomalous example of locomotor ataxia, or of muscular anæsthesia. At the time I first examined him, the throat symptoms (spluttering, etc.) had disappeared, and he could swallow fluids easily. There was, moreover, no history of diphtheria. He was ordered mustard blisters to the neck, inhalations of steam, and, on account of the affection of the limbs, five minims of liquor strychniæ three times a day. For upwards of a week he appeared to improve slightly, although on one day (the 12th) he again found himself for a short time unable to swallow fluids. The application of galvanism, which was tried on six or seven occasions, he also thought beneficial. On the 27th, however (more than three weeks after his admission), he became worse; his feet were cold, sensibility to touch and pain were diminished, the limbs were so weak that he could not walk the length of the ward, and his hands and arms were so incapable that he could not button his coat, nor take a good hold of anything, large or small. These symptoms increased in severity, and on the 30th he fell, in attempting to walk. He was ordered nourishing diet, meat for breakfast and dinner, and eggs to tea, with a pint of porter. He was also ordered cod-liver oil, and some days later half a grain of nitrate of silver was added. He was also desired to rest completely, and to be as little out of bed as possible. When lying in bed he could not move his limbs, or did so only with the greatest effort. The æsthesimeter showed that the sensibility of the skin was diminished everywhere, but specially in the legs and forearms,—above all, in the right leg.

In this state it was very apparent that the case was not such as I had supposed it on admission, and when, about the middle of January, some improvement was manifested, the paralysis of motion and sensibility simultaneously diminishing, I suspected that the symptoms might be due to diphtheritic poison; and, on making inquiry, made out some of the points in connexion with the sore throat, which I have detailed in the commencement of my narrative. I wrote to Dr Brown, of Berwick-on-Tweed, under whose care he had originally been, inquiring as to the nature of the sore throat, and indicating my reasons for suspecting diphtheria. I received the following satisfactory reply:—

"My dear Sir,—I attended J. Roxburgh for diphtheria some months ago, at least I considered it so, and treated him accordingly. He was some seven days under my care for it. Some time before and a little after I had several cases of the kind occurring in children in the near neighbourhood. . . . . I have not the slightest doubt you are correct as to its being diphtheritic. About the time I attended Roxburgh I find, on referring to my book, that I also attended other three adults for the same disease. In none of my cases remaining here has any paralytic affection appeared.—Believe me, my dear Sir, yours sincerely, COLVILLE BROWN."

The nature of the case was thus rendered fully apparent, and the prognosis correspondingly improved. Day by day he has progressed. He grasps objects firmly, and can write as well as ever. Sensibility and motion have gradually returned. He has made a trial trip on his engine with complete success, and walks with perfect steadiness.

On being satisfied as to the nature of the case, I substituted some compound tincture of cardamoms and aqua menthæ piperitæ for the nitrate of silver, evidently without disadvantage to the patient.

About the middle of last June, Mr M., a medical student, aged 17, consulted me on account of general debility and unsteadiness of gait. The following outline of his case is mainly derived from notes supplied by himself:—

In April, he had a severe attack of ulcerated sore throat, which, though regarded at the time as non-specific, must undoubtedly have been diphtheritic from the character of the symptoms which followed, and from the fact that one of the patient's brothers, living in the same house, had just recovered from diphtheria.

Soon after his recovery, Mr M. began to suffer from paralysis of the soft palate, indicated by a tendency for the liquid portions of the food to enter the nose; he was also subject to fainting fits on making any unusual exertion. These symptoms, however, soon disappeared with the improvement in his health, which seemed to be almost completely reinstated, though great weakness still continued.

Along with these symptoms, I should have mentioned, he began to suffer from defective vision, caused by an almost complete paralysis of the circular fibres of the iris, and of the muscles of accommodation. For these he consulted Dr Argyll Robertson, who prescribed a solution of Calabar bean in glycerine, which speedily effected a cure.

About the middle of June, Mr M. first experienced a sensation of numbness in the feet and legs, and want of power in the muscles of the lower extremities, rendering it impossible to stand on tiptoe, and extremely difficult to flex the leg and thigh, as, for instance, in going up a stair. These symptoms first appeared, he believes, on his taking a long walk during a botanical excursion.

When he first consulted me, he was quite unable to walk or even stand with his eyes shut, but with his eyes open he could walk tolerably well. His general health was good, excepting that he was anæmic.

I prescribed six minims of liquor strychniæ three times a day, and this seemed to produce partial relief, but as he continued to exert himself, in the endeavour to attend his classes, which necessitated a walk of two miles every day, he soon began to grow worse instead of better, and the numbness and loss of power appeared in the forearm and hand also.

In the beginning of July, Dr Robertson and I saw him together, and agreed to try him with small doses of nitrate of silver. Mr M. was obviously unable to continue his studies, and left Edinburgh for Penzance, making the journey, as far as practicable, by sea, so as to avoid the vibration of railway-carriages. The sea voyage produced a marked improvement, which continued to increase in the healthy climate and fresh sea air of Penzance, so that about a week after his arrival there he was able to walk a short distance without the least inconvenience. He then commenced bathing in the open sea, and the effect which this produced was, as he says, almost magical. About a week or so after he began it, the numbness had entirely disappeared, and he was able to walk with his eyes shut, and to stand on tiptoe, and before he left the seaside, after about a month's residence there, he could walk seven or eight miles, or row for an hour without fatigue. In fact, his health and strength were perfectly reinstated.

At the time that Dr Robertson and I examined him together, we were greatly struck with the severity of the ataxic symptoms, and these continued to be the most prominent symptoms during the time he was under my observation.

On comparing these cases with the general clinical history of examples of diphtheritic paralysis, certain marked peculiarities cannot fail to attract attention. Permit me, in a few words, to indicate the features of an average case for purposes of comparison.

The first symptoms of paralysis almost always appear two or three weeks after the subsidence of the local affection, and begin gradually to develop themselves while the patient seems to be making a perfect recovery.

The earliest symptom is generally paralysis of the soft palate, manifesting itself by a peculiar nasal tone of voice, and a tendency for liquids to regurgitate through the nostrils. The paralysis next attacks the legs, occasioning an unsteadiness of gait; the arms, interfering with all the finer movements of the hands, and in many cases even occasioning complete suspension of motor power; or the eyes inducing squint, with confused vision or loss of accommodation; or, it may be, of the muscles of the neck and trunk. Sensation is sometimes first affected, sometimes motion, frequently both simultaneously. In certain cases, of which those under consideration afford examples, there is distinct ataxia. Severe pains not unfrequently occur in the joints, and distressing formication is common. The bladder and rectum, though rarely, are occasionally involved. Of all internal organs, the heart is most seriously and frequently affected. Its action becomes tumultuous, but weak, while the pulse is slow. Fainting fits are not uncommon, and death sometimes results from syncope. Anæmic murmurs are often audible in the large vessels.

The symptoms having reached their utmost severity, in a great majority of cases improvement sets in, and recovery becomes soon complete.

The duration is from two to eight months.

As to the frequency of the different symptoms, the following table, from Dr Maingault's work,[1] may afford sufficient evidence.

He found, in 90 cases of diphtheritic paralysis,—

| | |
|---|---|
| Paralysis of the soft palate, . . | 70 times. |
| General paralysis, . . . . . | 64 ,, |
| Amaurosis, . . . . . . | 39 ,, |
| Paralysis of legs, . . . . . | 13 ,, |
| Strabismus, . . . . . . | 10 ,, |
| Paralysis of muscles of neck and trunk, | 9 ,, |
| Loss of sexual excitability, . . | 8 ,, |
| Loss of sensibility without paralysis, . | 8 ,, |
| Paralysis of rectum, . . . . | 6 ,, |
| Paralysis of urinary bladder, . . | 4 ,, |

[1] De la Paralysie Diphtherique, p. 14.

Although, in the cases I have detailed, the ataxic symptoms were by far the most striking, there were some others of which I should like to say a word, viz., the affections of the throat, the voice, the heart, and the eyes.

1. *The Affection of the Throat.*—This, the most common nervous sequel of diphtheria, presented a peculiarity in the case of the engineer, in that it recurred on several occasions, and lasted upon each only a few hours. It appeared first during the diphtheritic attack, recurred a fortnight later, and again on two occasions about the eleventh or twelfth week of the disease.

2. *The Voice.*—There was no trace of nasal twang in either case; but the engineer stated that at one time, for a period of a fortnight, his voice became distinctly treble—a statement which was confirmed by some of his friends. I have found it impossible to suggest a satisfactory explanation of this fact, but have no doubt of its truth.

3. *The Heart.*—The action of this organ was not altered from the healthy condition in the case of the engineer, but the medical student's pulse was always slow and feeble, and on several occasions he had attacks of syncope.

4. *The Eye.*—In both cases there was impairment of vision. The history led me to conclude that at one time the engineer had suffered from partial paralysis of the third pair, as he had been affected with ptosis, and a misdirection of the eyes, accompanied by confused vision. The student suffered from marked paralysis of the ciliary branches of the third pair, inducing a dilated state of the pupil and loss of the power of accommodation. The third pair of nerves is known to be peculiarly liable to diphtheritic paralysis, and especially its ciliary branches. The ptosis and squinting I at one time surmised might be early symptoms of true locomotor ataxia.

*Lastly*, As to the point to which I wish to call special attention—the ataxic symptoms. In the case of the engineer, these appear to have manifested themselves early, for they attracted his attention within about three weeks of the commencement of his illness, and constituted for nearly ten weeks his most prominent symptom. During this time his motor power and cutaneous sensibility were unimpaired. Any physician who considers the symptoms as I have described them, and remembers that there was apparently no history of diphtheria, will perceive how puzzling the case necessarily seemed at this time. In the case of the medical student, the ataxic symptoms were also very prominent, and, in proportion to the whole duration of the disease, were long continued. Both Dr Robertson and I were struck with the characteristic unsteadiness of his gait.

While it would be scarcely possible for one thoroughly familiar with the disease, or supplied with a history of diphtheria, to fail to distinguish such cases, it seems to me at least as necessary to form rules for differential diagnosis between this form of diphtheritic affection and other ataxic diseases, as it is to give rules for distinguishing those which are commonly dwelt on in the books.

The points which ought to be borne in mind are the following:—

1st. The history of sore throat in the diphtheritic affection, contrasting with that of sharp-shooting neuralgic pains and other prodromata in progressive locomotor ataxia.

2d. The existence of throat paralysis indicated by nasal tone of voice and by dysphagia, especially of fluids, occurring only in the diphtheritic.

3d. The dilated pupils and paralysis of accommodation not occurring in any excepting the diphtheritic.

4th. The suddenness with which, after diphtheria, the ataxic symptoms become developed.

5th. The gradual superaddition of paralysis of cutaneous sensibility and of motion.

## ARTICLE VIII.—*On the Action of the Cobra Poison.* By J. FAYRER, M.D., F.R.S.E., C.S.I.; Surgeon, Bengal Army; Professor of Surgery in the Medical College of Bengal.

(*Continued from page* 821.)

### FOURTEENTH SERIES.

Present—Dr Fayrer and Mr. Sceva.—10th July 1869.

EXPERIMENT No. 1.—A large and powerful pariah dog was bitten in the thigh, by a daboia Russelli at 3.22 P.M.; the dog showed signs of pain when the fangs penetrated. 3.25.—Walks, but drags the bitten limb. 3.28.—Is lying down; on rousing the dog he is unable to stand; defecation and micturition occurred; shows no signs of suffering beyond occasionally a suppressed whine; tries to stand, but is unable to do so; contents of bladder dribbling away. 3.32.—Respiration hurried; pupils dilated; rolls his head uneasily, but keeps the neck turned more to one side; twitching of eyeballs; stretches out the fore-legs in a convulsive manner. Lies otherwise quite paralyzed. 3.35.—Breathing regularly, but lies motionless. 3.38.—In the same condition; respiration forty in a minute; slightly raises his head at intervals. 3.45.—Still breathing, but lies perfectly still, giving occasionally a low suppressed whine. 3.53.—In the same condition; has watery purging. 4 P.M.—In the same condition; respiration forty-five in a minute. 4.7.—Can just raise its head when roused, the limbs seem quite paralyzed. 4.9.—Muco-sanguineous purging; other symptoms the same. 4.18.—Still breathing; more muco-sanguineous purging. 4.20.—In the same condition. 4.40.—In the same position; lying on his side; legs extended; breathing still. 4.45.—Slight twitching of the muscles generally; respiration irregular and feeble. 4.50.—Dead; a slight tremor, but no convulsive movement, preceded death. Bitten at 3.22 P.M.; dead at 4.50, or in eighty-eight minutes. The body was examined one hour and twenty minutes after death. The lungs were not congested. The liver was darker coloured than natural. The blood in the heart and great vessels was perfectly fluid, nor did it coagulate when collected and set apart. I examined the blood at noon on the 11th July most carefully and deliberately under the microscope, with a high power. There was no change. The red and white corpuscles were in their natural relative quantities; a very few of the red ones were crenate. But there was not a trace of any new cell or molecular matter in the blood. The perfect and permanent fluidity in the blood was remarkably illustrated in this experiment.

EXPERIMENT No. 2.—A pariah dog was bitten at 3.28 P.M. in the thigh by a large bungarus fasciatus said to be quite fresh, and about four and a half feet long; the bites drew blood. Walking about; drags the leg slightly. 3.34.—Looks depressed and is salivated. 3.36.—Walks about; looking scared. 3.40.—Bitten again in the thigh by the same bungarus; the dog evinced no sign of suffering. 3.42.—Looks dejected; foaming at the mouth; salivated. 4.7.—The dog is sick, and vomited a quantity of frothy mucus; vomiting repeated frequently. 4.10.—In walking he looks depressed, as though excessively nauseated, and limps on the bitten leg. 4.12.—Vomiting continues; lies down for the first time; breathing hurried. 4.17.—The nausea and vomiting continue; looks scared and depressed. 4.20.—Excessive vomiting of frothy mucus. Lies down; is convulsed in the hind-legs; looks very ill. 4.29.—Hurried catching respiration; twitching of the hind-legs. 4.32.—Walking slowly and feebly, with a dejected look; vomits frequently, and froths profusely from the mouth. 4.33.—Stands with his head drooping; still very sick; leans his body for support against the wall. 4.45.—No change. 5 P.M.—Appears better. 5.15.—Looks better; no vomiting; respiration more natural. 5.40.—Lying down; when raised on his feet, appears weak, but otherwise better. On lying down, arranged his legs in a natural position as if for sleeping. 6.10.—On being again roused, he walked about; his legs appeared feeble at first, but appeared to recover the use of them. 9.15.—Sleeping comfortably; on being roused, looks brighter and intelligent. 11*th July*, 6 A.M.—Remained during the night without changing his position; on being placed on his feet appears weak, particularly in the hind-legs; he appears somewhat numb in the legs.

I received the following report on the 13th July:—

"The dog died at about 10.33 P.M. of the 12th. Bitten at 3.28 P.M. of the 10th July; dead at 10.30 P.M. of the 12th, or in about fifty-three hours. Yesterday morning (the 12th) I observed that he was very weak. During the day, and up to the time of his death, he remained lying on one side, with the legs extended, passing at intervals muco-sanguineous matter. On opening the body this morning, I found the blood coagulated in the heart and great vessels." The blood sent to me on the 13th was firmly coagulated.

Under the microscope, it presented innumerable needle-like crystals of hæmato-globulin. The red corpuscles visible were very few in number, and were not, so far as I could judge, changed in any way. But I would speak with reserve about the corpuscles of this blood, as the field was so entirely filled with the crystals that little else could be seen, even after careful dilution with water and agitation. It is possible that new cell forms may have been there, and escaped detection. The mass of the red corpuscles seem to have been converted into crystals. In both this and the preceding case, the blood was examined some time after death, but I failed to detect any new cell growths.

EXPERIMENT No. 3.—A young cobra, about ten inches long, was bitten at 3.45 P.M., by a fresh full-grown cobra (*keauteah*) near the tail, so that the viscera might not be injured. The fangs were seen to penetrate, and no doubt could exist that the poison was fairly inserted. Being put on the ground, it crawled away vigorously, seemed unaffected by the bite. 5 P.M.—No change. 11*th July*, 6 A.M.—No change; it is quite well and active. On the 13th July, I saw it quite well. On the 17th, it was found dead; apparently it had been dead about twelve hours.

EXPERIMENT No. 4.—Another young cobra of the same brood as the last (No. 3) was bitten by a fresh daboia near the tail like the last. The fangs penetrated, and the poison was freely inserted. 5.10.—No change. 6.15.—No change, except that, when moving about, the end of the tail beyond the part bitten appears stiff, and does not move so freely as the rest of the body. This is accounted for by the nature of the wound inflicted by the formidable fangs of the viper. 11*th July*.—No change. 13th.—The snake is alive and apparently well. On the 17th, it was found dead, and decomposed; it had probably been dead three or four days. These two young cobras were of one brood; they were caught a few days ago, and are said by the snakemen to be about a fortnight or ten days old. There could be no doubt about their having been fairly bitten by the cobra and the daboia on the 10th; no evil result followed up to the 13th, though they died subsequently. Surely this is strong proof that the cobra is but little, if at all, susceptible to the poison of its own species. These snakes being so young, may have died from want of food, and partly from the effects of the wound, independent of the poison. They were alive on the fourth day after being bitten.

EXPERIMENT No. 5.—A white half-grown kitten was bitten by a bungarus fasciatus, said to be fresh, at 4.9 P.M., in the thigh. It seemed much excited shortly after. 4.25.—Lying in the former position, stretching out the fore-leg in a convulsive manner. 5 P.M. —In much the same condition. 6.10.—It has been very restless; now seems inclined to sleep; appears to be free from pain. 9.15.— ppear now to be much affected by the poison. 11*th July*.

—It seems better. *13th July.*—The kitten was quite well. It was evident in this case, that the animal was not mortally, though thoroughly, bitten; for the snake was made to close his jaws on the part, and drew blood. This, I believe, is just the sort of case which probably frequently occurs when men or animals are accidentally bitten—enough venom is injected to cause symptoms of poisoning, but not enough to destroy life, and the man or animal recovers chiefly by his or its own inherent power of recovery. Had I administered any of the so-called antidotes, or injected any of the proposed remedies, the recovery might have been attributed to the means used. That a man or animal so poisoned may be benefited by the use of stimulants, or other therapeutic measures, I do not for a moment deny; but, as I have before said, this is a very different matter to that of administering an antidote that shall neutralize the poison, and by so doing save life.

EXPERIMENT No. 6.—Another kitten, of the same size and age as that in experiment No. 5, was bitten by a cobra in the left thigh, at 4.16 P.M. The bite was very imperfect, and was repeated at 4.20 P.M. 4.24.—The kitten very restless, and springing about violently. 4.25.—Hurried breathing; restlessness. 4.45.—Becoming weaker; respiration irregular. 5.5.—Convulsive movements generally. 5.20.—Dead, in one hour and four minutes. 6.20.—Body opened one hour after death. Lungs natural; no congestion; the blood, on being removed from the heart and great vessels, soon coagulated firmly.

EXPERIMENT No. 7.—A bungarus fasciatus was fairly and deeply bitten by a fresh cobra, at 4.27 P.M., near the tail; no doubt of the penetration of the fangs and inoculation of the poison. No effect was produced. The bungarus was well and active on the 16th, five days after the bite.

EXPERIMENT No. 8.—A bungarus fasciatus was thoroughly bitten by a fresh daboia, at 4.32 P.M., near the tail. No evil result followed; the bungarus remained unaffected; on the 16th July was in its normal condition.

Several facts of importance are proved, or their probability confirmed, by the preceding experiments. In death by poisoning by the daboia, and therefore, probably, by all the viperine order—viperidæ and crotalidæ—the coagulability of the blood is generally destroyed. I say generally, because, though frequently, it is not invariably so. In the experiment on the fowl, it was found that the blood had coagulated. It remains fluid after death on exposure to the air. The most careful and protracted microscopic examination could detect no structural change in the corpuscular elements of the blood. Death is more protracted, but the deadly effects of the poison are even more quickly manifested than in death from cobra-poisoning. In point of lethality both appear equally dangerous. In death by cobra-poisoning, the blood coagulates firmly after and even before death, as post-mortem examinations made at all periods, from immediately, to an hour or more after death, have shown the blood to be coagulated firmly. No changes in the corpuscular elements have been seen in any of the microscopic examinations I have made. The poison of the bungarus is less deadly than that of the cobra or daboia, but it is very dangerous, though it is slow in producing its worst effects. It also does not destroy the coagulability of the blood. Perhaps this may prove to be the case with all the poisonous colubrine snakes. No change was observed in the corpuscular elements, *i.e.*, of such as remained. But the red corpuscles had passed, in the case of the blood of the dog that died from a bungarus bite, into a state of excessive crystallization of a needle-like and long tabular form. It is very doubtful if the cobra and daboia are affected by each other's poison; but the evidence on this point is not yet complete. The bungarus is also less susceptible to the poison of the daboia and cobra than the *innocuous* snakes, if, indeed, it be affected at all. Death was not caused by asphyxia in any of these cases. Everything tends to show that it is due to direct exhaustion from paralysis of the nerve-centres.

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## FIFTEENTH SERIES.

### Present—Dr Fayrer and Mr Sceva.—17th July 1869.

EXPERIMENT No. 1.—A large and powerful pariah dog was bitten in the thigh, at 2.45 P.M., by a fresh cobra (*keautiah*). The hair had been previously removed from the part in order that the puncture of the snake's fangs might be distinctly seen. The moment the fangs were withdrawn, the punctures were scarified, and carbolic acid at once applied, and well inoculated into the bites. The tissues were whitened, and the blood coagulated by the acid. 2.53 P.M.—The dog looks depressed and dejected; hanging his head. 3.12.—Lying down; looks dejected, but perfectly intelligent. 3.15.—Respiration hurried. 3.23.—Pupils widely dilated. In convulsions, rolled over on the other side; respiration irregular and catching. 3.27.—Violently convulsed. 3.30.—Respiration has ceased, but the heart still beats distinctly. 3.31.—Dead, in forty-six minutes. The carbolic acid was evidently of no service in this case. Post-mortem examination at 5 P.M. Blood coagulated; no crystallization under microscope.

EXPERIMENT No. 2.—A fowl had the feathers removed from the thigh, so that the bites might be seen, and was then bitten there at 2.54 P.M. by a daboia. The wounds were immediately scarified, and the carbolic acid thoroughly applied to the bites. The fowl fell over in convulsions when released, and was dead in less than sixty seconds. The body was opened at 3.35, or in about forty minutes after death, and the blood was found to be coagulated in the heart and great vessels; some fluid blood escaped into the thorax. The lungs were not in the least congested. The condition of the blood was particularly noted, as it has generally been found fluid in the mammals dead from the daboia bite. Post-mortem examination of dog, experiment No. 3. Blood examined at 5 P.M.: fluid when removed, but coagulated on exposure to the air. Microscopical examination: no crystals; no change.

EXPERIMENT No. 3.—The poison of a fresh cobra (*gokurrah*) was taken from the snake in my presence, and ten drops of it immediately injected with the hypodermic syringe into a middling-sized dog's thigh, at 3.3 P.M. The tube of the hypodermic syringe was not removed; and the syringe being filled with carbolic acid, about twenty drops were injected exactly in the track of the poison, and in the shortest space of time possible. 3.8.—The dog is depressed; looks scared; hangs his head; twitching of the hind-legs when he is raised. 3.15.—Lying on his side almost paralyzed; pupils widely dilated. 3.20.—Is convulsed. 3.22.—General twitching of all the muscles of the body; is quite unconscious. Respiration has ceased, but the heart still beats distinctly. 3.25.—Heart still beats. 3.27—Irregular action of heart. 3.29.—Dead, in twenty-six minutes. In this case there could be no doubt of the perfect inoculation of the carbolic acid, for it followed the poison through the same channel, and in the shortest possible space of time in which any local remedy could be applied, and yet without producing the slighest benefit. The second of time by which the poison preceded it was sufficient to cause death; no remedy could have been applied more rapidly, unless it had been mixed with the poison and introduced with it; in which case the venom might have been probably decomposed and rendered inert. It appears to be impossible to overtake the poison and neutralize it when once in the circulation, however rapid may be the inoculation of the supposed antidote.

EXPERIMENT No. 4.—A fowl was bitten in the thigh by a daboia at 3.19 P.M. The carbolic acid was immediately applied to the wounds, which had been at once scarified. 3.19,30.—Fowl in convulsions. 3.20.—Dead, in one minute. Body opened. Blood in heart and great vessels had coagulated.

EXPERIMENT No. 5.—A small dog bitten in the thigh, by a bungarus fasciatus (one used last week), at 3.13 P.M. The bites drew blood. 3.29.—Looks dejected. 5.20.—No further change. 18*th July*, 7 A.M.—No change. At 12, noon, the dog appears very weak; has not altered his position (recumbent) since last report. 6 P.M.—The same; refuses food; gradually drooped throughout the day. 19*th July*.—Died at 1.35 P.M., in about 46 hours and 27 minutes. Blood examined at 7.45 P.M.; blood clotted after death firmly; the serum paler than usual; corpuscles natural; no crystallization.

EXPERIMENT No. 6.—A fowl was bitten by another bungarus, which had also been used last week, at 3.32 P.M. 3.40.—The fowl looks uneasy, but not otherwise affected. 18*th*, 7 A.M.—Crouching on the floor; wings drooping. Noon.—Unable to stand; profuse flow of watery blood from the beak. 3 P.M.—Lying on its side; eyes closed. 5.40 P.M.—Died in 26 hours and 18 minutes. Body opened at 6 P.M.; blood coagulated; under microscope no crystallization had occurred.

EXPERIMENT No. 7.—A fowl was bitten by a cobra in the thigh at 3.45,30. Ran about for a moment when placed on the ground. 3.46.—Crouched; resting its beak on the ground; fell over, and was dead at 3.47. Body opened at 5.10 P.M. Blood fluid, but coagulated on exposure to air.

EXPERIMENT No. 8.—A daboia was well bitten by a cobra near the tail, at 4.5 P.M. 18*th July*, noon.—No change. 18*th July*, 6 P.M.—No change; no effect was produced. The daboia was alive a week after being bitten.

EXPERIMENT No. 9.—A varanus flavescens was bitten by another daboia, at 4.15 P.M. The daboia had bitten before. He did not strike his fangs readily through the hard skin of the lizard. 18*th July*, 3 P.M.—No change. 6 P.M.—No change. The varanus was not affected; it was alive a week after being bitten. The daboia was not fresh; and it did not bite vigorously, hence the escape of the varanus.

EXPERIMENT No. 10.—A cobra was bitten by a daboia near the tail at 4.3 P.M., and was bitten again by another daboia at 4.8 P.M., near the same place. 18*th July*, 5 P.M.—No change; a week later, no change.

NOTE.—The bungarus bitten by the daboia, on Saturday, 10th July, was found recently dead on Saturday, 24th. Death may be due to natural causes. The bungarus bitten at the same time by a cobra is alive and well on the 24th July.

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## SIXTEENTH SERIES.

Present—Dr Fayrer and Mr Sceva.—24th July 1869.

EXPERIMENT No. 1.—In the experiments hitherto performed, the snake has been made to close the jaws on the part bitten, and not been left to strike in the natural way. With the object of ascertaining whether there be any difference in the effect of the compulsory and voluntary bite, the following experiment was performed; and I observed in this, as on other occasions, that the snake rather attempted to frighten than to bite the dog, and it was not until the cobra was much irritated by repeatedly bringing the dog near him, that he gave the fatal bite. It struck the dog twice in the hind-leg, apparently without any effect, but afterwards struck, and for a moment fastened on to the thorax. Two slightly bleeding points marked where the dog was bitten—this was at 3.32 P.M. 3.45.—The dog is affected; vomited and was purged; very restless. 3.50.—Vomiting and staggering as he walks. 3.53.—Convulsed. 3.57.—Heart still beating irregularly; respiratory movements ceased. 3.45.—Dead, in twenty-six minutes. Another object of this experiment was again to search in the post-mortem blood for the corpuscles described by Professor Halford.

The body was opened at 5 P.M., or about an hour after death. The blood coagulated firmly, and was repeatedly examined under the microscope with a high power; but I could detect no change whatever in its corpuscular elements. The lungs, as usual, were free from congestion.

EXPERIMENT No. 2.—Placed a ligature round a fowl's thigh, and tied it very tightly. The fowl was then bitten by a cobra, at 4 P.M., below the ligature. The ligature was tied as tightly as it could be drawn, and appeared to arrest the circulation completely, for the part below became livid, and the limb was paralyzed. 4.13.—The fowl lies quiet, and does not seem to be affected by the poison. 4.17.—Is active and lively; hops about on the sound leg. 4.20.—Does not seem to be in the least affected by the poison; at this period—that is, fifteen minutes after the bite—the ligature was removed. The limb was turgid and livid from congestion. The bird began to droop almost immediately the ligature was removed. 4.22.—Drooping; does not rise; when raised crouches again. 4.23.—Head falling over; can hardly move. 4.24.—Convulsed. 4.30.—Still alive; faint; convulsive movements continue. 4.33.—Dead. Bitten at 4.5 P.M.; ligature removed at 4.20, no effect of poison being manifested. Dead at 4.33—*i.e.*, thirteen minutes after the ligature was removed. The poison was slow of entering with the stagnant blood of the congested limb, but it did enter, and killed in thirteen minutes.

EXPERIMENT No. 3.—A fowl was bitten in the leg by a cobra, and a ligature tightened round the thigh immediately. This was at 4.10 P.M. 4.12.—Runs about; limping on the bitten leg, which is almost paralyzed by the ligature, and livid with congestion. 4.17.—Slightly affected; appears to droop a little. 4.27.—No apparent effect of the poison. 4.30.—Scarified the fang punctures deeply, and rubbed the carbolic acid well into the wounds until all the blood coagulated, and the scarified surfaces were whitened by the acid. 4.31.—Removed the ligature; the fowl hops about, dragging the wounded limb, but not affected, apparently, by the poison. 4.35.—Poison now beginning to take effect; the fowl, as it was running, staggered and fell forwards, it then crouched, and its respiration was very rapid. 4.40.—Cannot walk; when raised falls over again. 4.42.—Head drooping; eyes closing; beak resting on the ground.

It continued alive until 5.35; and during this time it showed distinct reflex action of legs if the feet were pinched, and of the wings as in flying when raised in the air. It remained lying on its side, and died at 5.35 P.M. Bitten at 4.10 P.M. Ligature applied immediately. Ligature removed 4.31, in twenty-one minutes. Dead at 5.35, or in sixty-four minutes after the ligature was removed.

These experiments are very interesting. They prove that the poison enters and proves fatal by way of the bloodvessels; that if a ligature be sufficiently tightly applied (the great difficulty), the entry of the poison may be much, if not altogether, prevented; and that probably the application of carbolic acid or other caustic agent to the part poisoned, if thoroughly ligatured, may do good by decomposing the poison in the blood in which it is mingled.

But that too much faith must not be reposed in the acid or cautery is proved by this and former experiments, where the poison was prevented from entering the circulation by means of the ligature, and was subjected to the action of the acid whilst so detained; yet, when the ligature was removed, and the stagnant circulation again restored, death followed from the slow absorption of the poison, which, notwithstanding the thorough application of the acid, yet retained enough of its deadly qualities to cause death.

A fowl bitten by a cobra generally dies in a few minutes. It will be seen by these two experiments how long the fatal event was delayed by aid of the ligature and acid.

EXPERIMENT.—*Monday, 26th July.*—The same ophiophagus elaps mentioned in former experiments was brought to me again to-day. No new fangs had been reproduced; and it looked thin and half-starved. The snakemen say that no new fangs have ever replaced those originally taken out. I obtained, by squeezing the glands, about four drops of a deep orange-coloured viscid-looking fluid of the consistence of mucus. I made a slight puncture on a fowl's thigh with a lancet, and with an ordinary pen inoculated some of this yellow fluid into the wound, at 11¼ A.M. For some time the fowl seemed unaffected, but in the afternoon I found it in a lethargic state, crouching, with the head drooping, and the point of the beak resting on the ground. It gradually drooped. There appear to have been no convulsions, and at 10 P.M. it was dead.

This experiment shows that the entire loss of the poison-fangs, and consequent comparative inactivity of the glands, does not deprive them of the power of secreting poison; though, no doubt, it diminishes the activity, and probably modifies the nature of the secretion. All other snake-poison that I have seen taken from the armed and vigorous snake, of whatever family, has been a clear limpid fluid. In this case, it was a deep rich orange colour, and of the consistence of mucus. It proves also, that on the first removal of the poison-fangs, the reserve fangs may also be removed, leaving the reptile disarmed for life. This is not always effected by the operation of the snake-charmers; for they well know, and occasional fatal accidents have proved, that a new fang takes the place of the old one. I have had the fangs carefully removed from a cobra, and am keeping him to see in what time the new ones come forward. In one instance, I found that, after a fortnight, a pair of new fangs had replaced those removed, but they were not thoroughly anchylosed to the maxillary bones. A daboia, whose large fangs were removed with the same object, died shortly after the removal of the teeth, but whether the death was due to the operation or to natural causes I am unable to say. There is at present a daboia that has not touched food or water for six months in my possession, every effort having been made in vain to make the snake eat or drink, and it is vigorous and vicious as when first caught; but its venomous powers are probably diminished. During this period, the daboia has frequently changed its epidermic covering, and there are occasional deposits of solid urinary excreta passed. It never moves unless roused, when it is very active and vicious. The only conclusion that I can come to is, that it may have obtained moisture from the damp air, and may have swallowed flies or cockroaches or other insects that have found their way into the cage. But it certainly has neither taken food nor water in any other way during this period.

(*To be continued.*)

## ARTICLE IX.—*Notes on the Pathology of Pneumonia and Hepatitis.* By C. MOREHEAD, M.D., F.R.C.P.; late Principal and Professor of Medicine at Grant Medical College, Bombay.

THERE is an important question relating to the pathology of the lungs and of the liver, not yet fully settled. It rests on the fact, that in the lungs there is the pulmonary circulation for the aeration of the blood, and in the liver the portal circulation for the secretion of the bile. The question is, Which capillaries are the seat of inflammation in pneumonia and hepatitis? To my mind the answer is clear, Those of the bronchial arteries and of the hepatic artery. The pulmonary and portal plexuses are doubtless also involved, but in a manner secondary and indirect.

I. Are the capillaries of the bronchial arteries or those of the pulmonary artery primarily engaged in pneumonia? If inflammation be an altered state of the nutritive processes of the affected part, then the capillaries concerned in inflammation must necessarily be those which in their normal state circulate arterial blood for purposes of nutrition. The capillaries of the bronchial arteries are the nutrient vessels of the visceral pleura and all the tissues of the bronchial tubes, the coats of the bloodvessels, the nerves and lymphatics, and the connecting areolar tissue of the lungs, and they are the seat of visceral pleuritis and bronchitis. On the other hand, the capillaries of the pulmonary artery convey venous blood to the air-cells, in order that the physical process of endosmosis and exosmosis may take place between the gases of the blood and of the atmospheric air; and this process between the air and the blood becomes a main force by which the circulation in the pulmonary plexus is maintained. Assuming that *arterial* blood is a necessary factor in nutrition and inflammation, the question of the nutrition of the air-cells is not between the capillaries of the bronchial arteries and those of the pulmonary artery, but between the former and the capillary commencement of the pulmonary vein.

It seems to me impossible to avoid the conclusion, that the blood of the bronchial arteries is their factor in nutrition and inflammation of the air-cells; because, 1*st*, the bronchial arteries are the channels of nutrition of all the other structures of the lungs, and the seat of inflammation in pleuritis and bronchitis, one or other of which, and often both, almost invariably accompany .pneumonia. 2*d*, Dr Waters observes, "With regard to the nutrition of the air-sacs, no doubt can exist of the pulmonary vessels forming the source from which it is derived; no other vessels exist in the sacs, and in some animals the absence of the bronchial arteries shows the capability of the pulmonary vessels for the purpose;" and subsequently, in observations on the morbid anatomy of pneumonia, Dr Waters arrives at the following conclusions:—(*a*) "That pneumonia consists of an inflammation of the walls of the air-sacs." (*b*) "That the bloodvessels involved in pneumonia are the branches of the pulmonary artery, which constitute the pulmonary plexus; and that the capillaries of the bronchial arteries are in nowise implicated, unless there be concurrent bronchitis, which is an addition to the pneumonia, and not an essential part of it."[1]

The statement that no capillaries of a bronchial artery exist in the air-sacs must mean that none have been seen there. Nor is it likely that there should be, when we remember that there cannot be inflammation of the air-cells without turgescence of the pulmonary plexus, and that injection of a bronchial artery in part, at least, fills the pulmonary plexus; therefore, did bronchial capillaries really exist in the air-cells, they are sure to be veiled by the large pulmonary plexus under the only circumstances in which their detection is practicable; still it is most probable that bronchial capillaries do not exist in the air-cells. But does it follow from this that the blood of the bronchial arteries is not the nutrient blood of the air-cells? Certainly not. How are the cornea of the eyes, the articular cartilages, and lamellated osseous tissue nourished? Surely the capillaries of the termini of the bronchial tubes and of the interlobular areolar tissue carry the blood near enough to the air-cells to admit of their nutrition by imbibition of the plasma.

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[1] Anatomy of the Human Lung, p. 202. Lancet, 23d December, 1865, p. 702.

There is also another reason for believing that the blood in many of the bronchial capillaries comes very near to an aerating surface. It seems certain that a direct communication exists between the bronchial arteries and the pulmonary veins; but the exact anatomical relation is not satisfactorily determined. May it, therefore, in this uncertainty, not be assumed, that when bronchial capillaries subserve nutrition of tissues so close to an aerating surface as to admit of the blood in them, after use, becoming at once oxygenated,—as, for example, the minute terminal parts of the bronchial tubes and the interlobular areolar tissue—this blood passes direct into a pulmonary vein to join that blood which has been aerated in a pulmonary plexus; but that the blood of bronchial capillaries more remote from an aerating surface passes in its venous condition on to the vena cava by the bronchial veins? But Dr Waters further argues, that the pulmonary capillaries are the nutrient vessels of the air-cells, because "in some animals the absence of bronchial arteries shows the capability of the pulmonary vessels for the purpose." This argument is best met by the statement, that the analogy fails, because it is with an animal provided with bronchial arteries that we are dealing; and, further, it may be applied with equal justice to the nutrition of all the other tissues of the lung as well as the air-cells, in man and other animals provided with bronchial arteries.

On the question of the nutrition of the air-cells, the following suggestion may also be offered. If the pulmonary plexus be the channel of the nutrient blood of the air-cells, how are these structures nourished during the last two months of intra-uterine life? It is said that during the fœtal circulation a small quantity of the blood from the right ventricle is circulated through the lungs by the pulmonary arteries; but on what proof this belief rests, I do not know: what purpose can such circulation serve? It cannot be necessary as a mere mechanical means of keeping the vessels patent, for no such means of keeping the bronchial tubes and air-cells patent is suggested. If it be answered, For the nutrition of the air-cells, then the following question arises. Assuming that it is true that in the fœtal circulation the blood propelled by the right ventricle almost exclusively consists of the absolutely venous blood of the descending vena cava, then it follows that the air-cells are the only structures in the fœtus which are nourished by blood entirely venous. Is this probable, when we remember that areolar tissue exists in greater proportion in the lung of the fœtus than of the adult; and that, consequently, in the former the blood circulated in the bronchial arteries must be proportionately large, and presumably sufficient to include the nutrition of the air-cells?

*3d*, As shall presently be more fully stated, the cessation in the circulation of the pulmonary plexus is nearly coincident with the cessation of the aeration of the blood; and as the cessation and aeration must take place at the affected air-cells at a very early period of their inflammation, it is a legitimate conclusion, that the blood in the pulmonary plexus, becoming stagnant, is no longer in a condition to be a factor either in nutrition or inflammation; from which it would seem to follow, that, if nutrition of the air-cells depended on the blood of the pulmonary plexus, gangrene, not exudation, would be the rule and not the exception in pneumonia.

*4th*, Though the capillaries of the pulmonary plexus are not those directly engaged in inflammation, yet their deranged condition has much to do with the pathology of pneumonia. It is inattention to this distinction which has led to the confusion which seems still to exist in regard to the morbid anatomy of pneumonia.

II. I would now apply these propositions to the explanation of some of the leading facts of pneumonia. The phenomena which attend on the first inspiration after birth, on asphyxia, vesicular emphysema, and other pathological states of the lung, teach the following facts: *1st*, The pulmonary capillary circulation is contingent on the process between the blood in the pulmonary plexus and the air in the air-cells being in action. *2d*, If this process—aeration—be stopped from defective air, from thickening of the walls of the air-cells, or the cells becoming filled with liquid or coagulated exudations, the pulmonary capillary circulation on such air-cell walls becomes languid, and speedily ceases; the blood distends the vessels, and stagnates in them. New additional blood is no longer sent in to the branches of the pulmonary artery which conduct to these inefficient air-cells, but it passes in excessive quantity into adjoining branches to be conveyed to the plexus of adjoining efficient air-cells, that it may be aerated there. If, however, the blood thus sent to these adjacent normal air-cells be greater in quantity than the extent of surface can readily aerate, dyspnœa is caused. Short and hurried respirations merely express the fact that all the air-cells of the lungs are not in action, and that the diminished extent of surface thus arising is being compensated for by a greater frequency of the respiratory acts. Difficulty of breathing is only experienced when there is want of harmony between the quantity of blood in the vascular system, and the quantity or quality of air in the air-cells. *3d*, When aeration is interfered with, but not altogether obstructed, from thickening of the walls of the air-cells, and slight serous effusion into the cells, then, the circulation in the pulmonary plexus being impeded but not altogether stopped, the capillaries become distended, but the blood does not as yet stagnate. This I believe to be the condition of the affected portion of the lung in the first stage of pneumonia—congestion. The inflammation continues,[1] the thickening of the walls of the air-cells increases, serous effusion and exudations fill the cells. Now aeration at these air-cells becomes physically impossible, the blood stagnates, and the circulation is completely obstructed in their pulmonary plexuses. Such is the state of the lung in the second stage of pneumonia—hepatization; the spongy structure has become solid, in part from effusion and exudation into the air-cells; but in part also from the stagnated blood distending the closely-set capillaries of the pulmonary plexus. It is this latter kind of consolidation, and the process by which it is effected, which seems to me to have been overlooked by pathologists; and yet it explains well, what is otherwise difficult to understand, how consolidation sometimes takes place rapidly, and how it is sometimes very speedily removed. It is evident, that if part of the solid condition of the lung has depended on pulmonary capillaries turgid with stagnated but not coagulated blood, and aeration becomes re-established in the affected air-cells by reduction of their inflammation, then the stagnated blood will at once be set in motion, and the consolidation dependent on it will speedily cease; we can hardly conceive a change of this kind taking place so quickly, if the factor in the consolidation and in the inflammation were one blood, and one set of capillaries.

My belief, then, is, that the pulmonary are not the inflamed capillaries in pneumonia, but that, consequent on inflammation of the walls of the air-cells, the action of these capillaries becomes deranged in the manner which has just been explained. This derangement constitutes the danger of pneumonia, as of all other forms of pulmonary disease. Moreover, this deranged condition of the pulmonary capillaries takes a part in producing the striking feature in the morbid anatomy of pneumonia—hepatization. In this view of the pathology of pneumonia, we have a satisfactory explanation of the difference between hypostatic consolidation of the lung and hepatization: in the latter, there is in the turgescence of the pulmonary capillaries the condition of hypostatic consolidation, but there are also the consolidating conditions derived from the direct products of inflammation. Further, this obstruction of the parts respectively played by the bronchial and pulmonary capillaries in the pathology of pneumonia is confirmed by certain facts in the symptomatology and treatment of the disease. For example, the amount of dyspnœa is always an expression of the degree of disproportion between the quantity of blood to be aerated and the extent of the aerating surface. In asthenic and plethoric persons, in whom the entire surface is required, pneumonia of limited extent is attended with marked dyspnœa. But when the quantity of blood has been for some time reduced, as happens in the asthenic states, the full extent of the aerating surface is not required, and part of it may become unfitted by pneumonia, and yet dyspnœa be hardly noticeable. In these statements we have the explanation of the latency or obscurity of the symptoms of impaired function of the lungs in asthenic pneumonia. The little urgency of cough in pneumonia has been generally noticed. Cough merely expresses the fact that there exists an obstacle to the free transmission of air to efficient air-cells; it is a forcible expiratory effort called into action to remove the cause of the obstruction. It is therefore a fair inference, that when air-cells become unfit for aeration, and venous blood is no longer sent to them, but is directed to adjacent efficient cells, then, whether there be or not obstruction in the bronchial tubes leading to the unfitted cells, there is no longer the same necessity for cough to clear them; hence its little urgency in the solidified lung of pneumonia.

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[1] If we reflect on the kind of changes which take place in the cornea consequent on inflammation, we can have no difficulty in understanding the kind of changes which must take place in the walls of the air-cells on inflammation, even though they be non-vascular, as the cornea is; and in being satisfied that they must be of a kind adequate to interfere with, and then to prevent altogether, the aeration of the blood.

Then, as regards treatment, it is important to remember that dyspnœa indicates a want of proportion between the blood and the aerating surface, and that it may be lessened in one of two ways, either by restoring the surface to fitness, *i.e.*, removing the inflammation, or by reducing the blood to the just proportion. Therefore it is very evident that dyspnœa may be relieved in pneumonia by bloodletting, without a corresponding reduction of the inflammatory action. In this fact we have the explanation of a statement long since made by Alison,[1] that auscultation may indicate a continuance and even an extension of the disease for a considerable time after the breathing has been effectually relieved by venesection; the fact being, that the inflammation was unabated, but the quantity of blood had been brought more into harmony with the diminished aerating surface. Again, with the view of inculcating free bloodletting in pneumonia, Andral[2] has said, and Watson[3] has repeated, that it is useful on a principle applicable to all inflammations, and also on the principle by which the exclusion of light is useful in ophthalmia, and rest in an inflamed joint. The latter part of this doctrine is, I apprehend, erroneous; for if, after the air-cells have for a time been the seat of inflammation, they become unfit for function, and no longer exercise it, bloodletting can be of no use to them by relieving them from function, as exclusion of light and rest are to the inflamed eye and joint. It does good to the healthy air-cells by relieving them of part of that excess of function which they have been required to assume. But the only way in which the loss of blood can benefit the affected cells is by reducing the inflammation in the manner (whatever that may be) in which other inflammations are reduced by the same means. The benefit thus gained will be increased not by repose of these air-cells, but by the resumption of function on their part setting the blood which had stagnated in the pulmonary capillaries again in motion.

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[1] Outlines of Pathology, p. 281.

[2] Clinique Medicale, vol. ii. p. 378.

[3] Lectures on the Principles and Practice of Physic, vol. ii. p. 91, 3d edition.

With these observations, I conclude my arguments in favour of the opinion that the blood in the capillaries of the bronchial arteries, and not that in the pulmonary plexus, is the factor in nutrition and inflammation of the air-cells.[1] But I may not pass from the morbid anatomy of pneumonia without expressing an opinion on another question, which has been at times, and is even still, debated—viz., whether the exudation takes place in the air-cells, or external to them. This seems to me a very simple question. In inflammation of serous membranes, exudation takes place chiefly from the free surface, but it may take place also, in slight degree, in the connective tissue of the attached surface. Why should it be otherwise with the walls of the air-cells? The chief exudation must necessarily be on the free surface—that is, into the cells—but there is no reason why it may not take place also on the attached surface in slight degree, wherever there exists space; or, in other words, wherever there exists connective areolar tissue, as between the lobules. It does not require the microscope to prove that the exudation is almost entirely into the air-cells. In what respect does a lung compressed by pleuritic effusion differ from a lung in the full expansion of inspiration? In the former, the pulmonary vessels and the air-cells are empty, and their walls are pressed together; in the latter, the pulmonary vessels are filled with circulating blood and the air-cells with air. In what respect does a lung in the state of full inspiration differ from a lung in the state of hepatization? In both, the cavity of the chest is fully expanded; in inspiration the pulmonary vessels are filled with circulating blood; in hepatization they are filled with stagnated blood. In inspiration the air-cells are filled with air; in hepatization they do not contain air, yet they must be filled with something, otherwise the lung could not be fully expanded. Can there be any doubt that this something is the effusions and exudations of inflammation? Surely there is a close analogy between pleuritis and pneumonia. The pleura does not differ anatomically from the walls of the air-cells, except in its greater proportion of fibrous and connective tissues. In both there is very much the same kind of free surface—a pavement epithelium resting on basement membrane. In pleuritis there is effusion of serum and fibrinous exudation on the free surface; and in pneumonia we have exactly the same products of inflammation on the free surface of the air-cells. But in pleuritis (sthenic) the fibrine is organized into areolar tissue, and leads to abnormal adhesions, while in pneumonia (sthenic) the fibrine softens into pus or other liquid products. What is the cause of this difference? In pleuritis the exudation takes place into a sac closed from the atmospheric air, but in pneumonia into sacs open to the air. It was a remark, I think, made by Laennec, that abscess is not a result of acute pneumonia, because the patient does not live long enough to admit of the process being gone through. But may not better reasons be assigned? (1.) The influence of the air on the exudation. (2.) The fact that a hepatized portion of lung is never bounded by healthy structure, but always by a portion in the first stage of pneumonia—congestion; hence it is not placed in circumstances which admit of an outer layer of fibrine becoming organized into encysting areolar tissue.

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[1] When I first wrote on pneumonia in 1853 (Transactions of the Medical and Physical Society of Bombay, No. ii., new series, and afterwards in Clinical Researches on Disease in India), the only author who, to my knowledge, had noticed this question was Dr C. B. Williams, in the article "Pneumonia," in the Cyclopædia of Practical Medicine. I may have overlooked others then, and there may have been others since, previous to Dr Waters, but I am not acquainted with their writings.

III. *Hepatitis.*—I shall now, in conclusion, notice very briefly the analagous question in respect to hepatitis. Which are the capillaries concerned in inflammation? As the capillaries of the hepatic artery are the nutrient vessels of the liver, they are consequently those which are directly engaged in hepatitis; on the other hand, the portal capillaries circulate venous blood for purposes of secretion, not nutrition; therefore they are not directly engaged in inflammation, but are the chief seat of congestion. It is only by noting this distinction that we can rationally explain the pathology, symptomatology, and therapeutics of hepatitis. *Firstly,* The capacity of the hepatic capillaries is small compared with that of the portal capillaries, hence the fact that the bulk of the liver may be little increased in inflammation compared with congestion. *Secondly,* It explains why the secretion of the liver is often not deranged in hepatitis. *Thirdly,* It tends to remove that difficulty which practical writers (Abercrombie in particular) have experienced in reconciling the results of clinical observations with certain general principles in therapeutics. It has been urged, that to increase the secretion of the liver by cholagogue medicines in hepatitis is opposed to the doctrine that the special stimulants of secreting organs are contraindicated in acute inflammation of these organs. But this doctrine, doubtless true when the secreting and inflamed capillaries are the same, is not applicable to an organ such as the liver, in which the sets of capillaries are distinct. If it be true that the capillaries of the hepatic artery pass into the portal vein, then, to quicken the portal capillary circulation by increasing the secretion of bile is, in theory, a rational method of lessening the stasis of blood in the inflamed capillaries of the hepatic artery.

## ARTICLE X.—*On the Treatment of Delirium Tremens by the Hydrate of Chloral.* By GEORGE W. BALFOUR, M.D., Physician to the Royal Infirmary.

TO a reflecting mind there is nothing more interesting or more important than the change which within the last five-and-twenty years has come over our mode of treating delirium tremens. Formerly, under the influence of the dogma that the patient must sleep or die, the formula in vogue in this hospital was—℞ tinct. opii ʒi., tinct. hyoscyami ʒii., spt. communis ℥i., ii. p. r. n., till sleep was induced; and there can be no doubt that this treatment was most positively injurious in all but the most wary hands, and only kept its ground by being less hurtful than the indiscriminate practice of bloodletting which immediately preceded it. Subsequently, the expectant treatment recommended by Dr Ware was largely employed, and gave occasion to a free use of the padded cells originally provided for a very different class of patients. Less positively injurious than previous modes of treatment, the patients thus treated yet were exposed to various risks from their own violence, as well as from constitutional exhaustion, exposure to cold, etc. The introduction of the bromide of potassium, and its recognition as a safe and reliable nervine sedative, introduced a new era into the treatment of delirium tremens, and, instead of waiting and watching through a tedious convalescence, during which the patient was exposed to various risks of death, from twelve to twenty-four hours' treatment was quite sufficient to induce refreshing sleep and a speedy convalescence. Of course, failures of this treatment occasionally occurred, as they must occur in all kinds of treatment, chiefly, however, in patients exhausted by previous disease or maltreatment, or affected by epileptic convulsions; but the results as a rule were extremely satisfactory. Only in very severe and exhausted cases was it ever found necessary to conjoin the bromide with stimulants, and in them the convalescence was always tedious and unsatisfactory. On the other hand, many cases treated at home in vain by bromide of potassium plus stimulants were readily cured in hospital by the bromide without the stimulants. One of the most remarkable of these was sent in by a medical friend, who, regarding the case as a very serious one, anxiously inquired, if the patient lived, if he could possibly be removed at the end of a fortnight, as his passage to America was secured, and his friends were anxious to send him abroad. He smiled incredulously when told, that if he called back next day at the same hour, he would find the patient either well or asleep, and that he would be fit to be removed by the end of the week. The result was, however, as predicted, and my friend is now a firm believer in the air of Ward X., though still a little sceptical as to the bromide. The dose of the bromide was, however, large—half a drachm—and required to be administered frequently—every hour, often till ten or more doses were given—before it took effect. This frequent repetition of the dose was always irksome, and often troublesome; and having had considerable experience of the hypnotic power of the hydrate of chloral in the general wards, I determined to give it a fair trial on the earliest opportunity. The first case of delirium tremens treated by it was one of maniacal ferocity. He had been treated at home three days, and was regarded as most dangerous:—

"My dear Balfour,—The man Wallace had been drinking for about three weeks, was under treatment for three days before being sent to the Infirmary; had tartar emetic, Dover's powder, and camphor without effect. Was so violent that he threatened to kill somebody; sent to the Infirmary; the rest you know. Second or third attack, I think. Had not slept for three nights.

"The woman had recovered from puerperal convulsions; and two days before she was sent in, took mania, and would not take the chloral; was so violent that I had to put her under chloroform to take to hospital. Mania and constant wakefulness till she got the chloral.—Yours, etc., "JAMES A. SIDEY."

Two doses of hydrate of chloral, of thirty grains each, with an interval of one hour between them, sufficed to induce refreshing sleep and to restore the patient to health. The woman referred to by Dr Sidey was the next case submitted to its influence, and was cured by one dose of forty-five grains, which sufficed to induce calm sleep and restore the patient to apparent health. Since the 1st of February, when I commenced my annual service in the wards set apart for delirium tremens, there have been successfully treated eight cases of true delirium tremens, four of acute alcoholismus, and nine simply drunk, besides one case of puerperal mania, and one case of acute mania, without one fatal result. Through the kindness of Mr Hirschfeld, the resident physician, I am enabled to append one or two cases as examples of the action of chloral in such cases.

CASE II.—Admitted to Ward X., 5th March 1870.—Patient very violent; got a dose of 45 grs. chloral hyd. Soon it was seen that he was drowsy, but he fought against this inclination, and when he was almost asleep, he started up with a roar. In about one hour and a half, got a second dose of 30 grs. This in about twenty minutes made him sleep. He slept for about four hours, and then was almost quite sane. He spoke to me sensibly and quietly, and the straps were now taken off.

CASE III.—Admitted to Ward X., 24th March 1870, in a state of busy and furious delirium. She was put to bed, and strapped down. About 9 P.M., got chloral 45 grs., which produced no effect within half an hour, and then 30 grs. more were given. In about eighteen minutes after this, patient was sound asleep. She had slept an hour, when she was roused by the noise made by other patients being brought in, and did not fall asleep again. I saw her about 12, when she was about as bad as she had been the previous night; 30 grs. chloral were ordered, and to be repeated in an hour if necessary. The repetition was required, but after it she slept that afternoon, and was sane late that night, and next day was up. She was to have been dismissed next day, but her husband, afraid she would again plunge into former company, asked that she might be kept till Thursday, when he would take her north.

Case IV.—Admitted to Ward X. this morning, 2d April, having been brought in by the police. He had just had an epileptic fit, and his tongue was bitten. On admission he was asleep. Patient has been here before with epilepsy and drunkenness, etc. During the afternoon he was wild, and had seven epileptic attacks, and then was in a state of delirium tremens; was tied down to the bed. He was noisy and very restless. At 12.40 P.M., got chloral 30 grs., which seemed to make him wilder, for now he sang songs well, while he spouted with great vigour. This continued till 1.20 A.M., when 30 grs. chl. more were administered, and within seven minutes he was sound asleep. Three minutes later he sang one verse of a standard song, then lapsed again into quiet slumber. The nurse was told to waken me if he woke, but was incapable. He did wake and sing. At 12, got chl. 20 grs.; at 4, got chl. 30 grs.; slept all afternoon, once or twice sang, but immediately was asleep again, and made a good recovery.

Case V.—A woman who had been drinking for no one knows how long, was brought into the Infirmary, 8th April 1870. She shook very much. She had vomited her food for some time, but for the past three or four days she had vomited everything she took immediately after swallowing it. As I had seen chloral useful in stopping vomiting, I determined to try it here, and accordingly gave 30 grs. chloral in ℥iij. She vomited this soon. I then gave 15 grs. chloral by enema. This seemed to quiet her, and after a few hours she fell asleep, the first for some days.

*9th April.*—Took her breakfast and did not vomit it, nor did she vomit anything she ate this day. As she is still shaky, ordered dose of 30 grs. chloral, and was asleep in twelve minutes.

*10th.*—Is almost quite steady. No more vomiting, and takes now her food well. After this she continued steadily to improve.

Case VI.—Admitted from the Surgical, 9th April 1870. He has a fractured tibia and fibula, and is very delirious. At 11 P.M. got dose 45 grs. chloral; 12.40, 30 grs. chloral; 1.30, 30 grs. chloral; 1.40, delirium began to abate for a few seconds at a time, but still, with exception of these few seconds, he struggled hard, but by 1.45 patient was sound asleep and snoring. Now you could touch his pupil pretty roughly without any contraction, and he was pricked with a pin, only wincing slightly when it was stuck in pretty far. He did not waken, and soon snoring stopped, and he breathed softly.

10*th April*.—Awoke partly at five this morning, at seven completely. He was not altogether sane. He wanted up, and would not believe he had a broken leg. He would have been up had he not been tied down, but he did not struggle. Drank a good deal of milk—ate little else. 11 A.M. chloral 40 grs., asleep in fifteen minutes; awoke at 2 P.M. perfectly sane. Slept at first somewhat stertorously; asleep again between four and five, and slept all night; after this he progressed favourably.

The cases admitted into this ward vary from the merely excited fidgety condition, known as the horrors, to the most exaggerated mania, often accompanied by repeated epileptiform convulsions. The cases given are amongst the severer ones treated; the milder ones succumbed more readily to the treatment, one dose being usually sufficient; the general result being to keep the wards almost empty, from the rapidity with which the patients are enabled to be discharged. In regard to the various physiological points involved in the action of chloral as to its influence on the pulse, the respiration, and the temperature, experience is, I believe, scarcely as yet ripe enough to pronounce any definite opinion; and that, all the more, that its action seems considerably modified by the previous or concomitant administration of other remedies, the influence of such combinations requiring to be eliminated before we can obtain the pure results of chloral. In some cases hyperæsthesia seems to result; in most, however, various degrees of anæsthesia are produced. In two cases I could, at the first application, touch the cornea and roll about the eyeball in the fingers for a second or two without sensation being evinced; but this speedily recurred, and the depth of the anæsthesia seemed entirely to depend on the force of the hypnotic action—*i.e.*, on the largeness of the dose.

In conclusion, I may remark that it is now nearly forty years since Ware first pointed out that delirium tremens was a disease which ran its course naturally in from sixty to seventy hours; and that it was safer to allow it to do so than to treat it by the remedies then in vogue—large bloodlettings or poisonous doses of narcotics. It was more than twenty years before these doctrines became sufficiently spread abroad to produce much fruit; and in the most recent publication on the subject I am acquainted with, by Dr Cuming of Belfast, in the February number of the "Dublin Quarterly Journal of Medical Science," it is stated that the doctrine of the definite course and duration of delirium tremens ought to make us slow to entertain any extravagant expectation of materially abbreviating its duration by therapeutic measures. Coupling these words with what we know, and what I have endeavoured to show, as to the treatment of delirium tremens by bromide of potassium, and especially by chloral, we obtain this most important lesson, that our power to cut short and cure disease is limited, not so much by the nature of the processes involved as by our ignorance of the means to be employed, and that a brighter future for therapeutics may yet be looked for, when we shall have shaken ourselves free from the idea that the processes of nature cannot be successfully interfered with by art. Nature cures only in certain circumstances and under certain conditions, but art can change the one and fulfil the other. How this may be, science slowly and with difficulty explains; but empiricism is — though often opposed — usually in advance of science.

# Part Second.

## REVIEWS.

### *Report on the Cholera of* 1866–68 *in the Bengal Presidency, and its relation to previous Epidemics.* By JAMES L. BRYDEN, M.D., Surgeon, Bengal Army.

THIS Indian blue-book contains 245 pages of closely-printed matter, in which will be found abstract histories of all the cholera outbreaks which have occurred in Bengal since the beginning of the century; several maps to show the districts occupied by cholera at certain times, and an immense number of tabulated returns; and upon these are framed the views which Dr Bryden supports. We regret that the work is crudely composed; that there is often repetition and indistinctness; sometimes apparent contradiction, arising from assertions which have not been guarded so as to prepare us for subsequent admissions; and the direct vein of thought or argument with which the writer is engaged is often left to pursue, as it were, a lateral topic. With all these blemishes the work contains an enormous body of information and statistics, which, having been registered by men desirous only to note facts, must be most valuable in enabling us to approach the truth as to the outbreak and spread of cholera. We shall now endeavour to condense Dr Bryden's views, often giving the results only at which he has arrived, but, when of sufficient importance, giving also the reasons for those results.

Cholera, then, is an *earth-born* endemic disease, always prevailing more or less in what may be called the basin of Lower Bengal. This is a region of perennial moisture, whether we regard the atmosphere, the surface, or the subsoil. All the rest of India is the epidemic area. Cholera is extricated in the endemic area as soon as the breeding grounds rise above water, and here it appears again in strength, and becomes epidemic in February, March, April, and

May, when the permanent moisture causes vegetation to sprout forth into wild luxuriance.

But the endemic cholera does not always remain confined to its locality. In spring the moist south-east winds carry the disease over the brim of the endemic basin, either across the continent or up the Gangetic or Bramapootra valleys, its natural outlets. The first general fall of the rains extinguishes the disease in the endemic area, but fits for invasion of the epidemic every province lying within the influence of the monsoon. Where cholera does become epidemic, it does not at once spread over the country, but occupies successive areas—which may be called *natural provinces*—in which successive outbreaks repeat themselves constantly, and with precision, during each period of years, and that too in a measure independent of all other circumstances. Cholera, whenever met with as an epidemic in the state of invasion, depends on *moisture;* this alone supplying every requisite for the spread of the epidemic, without superadding any mysterious agency. The spring rains are of great importance in relation to the primary and secondary invasion of cholera; they prevail in the north of India, and the epidemic influence spreads coincidently with them. In the eastern epidemic area, moist east winds often blow, and the degree of prevalence and moisture determines the character of the season. In the south, also in the Saugor, Jubbulpore, and Nagpore territories, moist winds and rains from the east are common in the early months of the year; and if these regions are entered by epidemic influence from Bengal, it is not in the monsoon which then blows from the south-west, but from March to May, when the prevailing wind is from east to west, or north-east to south-west.

Cholera always spreads in the direction of the wind. There are only two instances in which it is alleged that cholera advanced against this rule: first, that it crossed the continent from Calcutta to Bombay in the teeth of the monsoon in 1818; and second, that it proceeded from Central Asia to Peshawur, Lahore, and Meerut, against prevailing winds, between 1843 and 1845.

In the first instance, however, the disease, after spreading from Chittagong, east to Balasore and Cuttack, and north-west from mouth of Ganges almost to its junction with the Jumna, finished with the camp of the Marquis of Hastings in November 1817. In the spring of 1818, the outbreak was universal in the eastern division of the epidemic area, and rapidly stretched across India to the west and south, taking only six weeks to reach from the Himalaya to Guzerat; Mhow being attacked on the 4th, Ougein on 9th, and Mehidpore on 12th May, being truly an advance with the prevailing wind of the season, about two months *previous to the setting in of the monsoon;* and when this cholera was afterwards revitalized, it was mistaken for the original invasion.

In the same way, consider that, although the cholera broke out in Central Asia before it appeared in Peshawur in 1844, it is not necessary to suppose that it travelled downwards; as, judging from the parallel outbreaks of other years, the normal period of the outbreak, which would appear in Peshawur, is later than the first, and the disease would have appeared at the fixed date independently of its presence to the westward.

Cholera has no locomotive power in itself, and its geographical distribution is essentially dependent on a *vehicle*, which is a moist atmosphere (cholera of a hot and dry season, whenever manifested, is a *reproduced*, not a *primarily invading* cholera). The wind is the agency which directs the course of the advancing epidemic, and determines limit in geographical distribution. After cholera has been extricated from the soil of the endemic area, in February and the following months it becomes epidemic, and the moist easterly winds carry it into the *eastern epidemic area.* Here, however, it is met, and, for the time, checked by dry hot winds from the north and north-west. The first monsoon rains (as mentioned above) check the cholera in the endemic area, but in Upper India the miasm is roused to epidemic life, and is ready to invade every district within range of the monsoon. Wherever the disease is met with in October and November, there will be found a meteorology special to the locality, causing the district to approximate, for the time, to the character of the Lower Provinces, or of that part of the epidemic area of the extreme east, which is apt to be affected in the same month. The cholera of any year or period is not a continuous manifestation, but is shown in a successive alternation of activity and dormancy; and there may be considered to be three periods: 1st, from the middle of February to May; 2d, from the 20th June to the end of September; and 3d, from the end of October to December; being the cholera of spring, of the rains, and of the cold season —each having a history and geography peculiar to itself. The homologue of each will be found in the various epidemic divisions of the presidency. Thus, spring cholera will be found in the Punjab and Nagpore as well as in Lower Bengal and Assam; the rains cholera in the endemic area and bordering countries, as well as in the monsoon area of Upper India; and the cold weather cholera in Rohilkund and the neighbouring terrai, as well as in the districts east of the Bay of Bengal.

A cholera endemic is a new emanation of cholera miasm from within the endemic area, covering, within a limited time, one, more, or all of the natural provinces; usually occupying a new province vacant (*free from cholera*) by the death of the last epidemic, and destined to continue (*live out*) the period normal to it in its epidemic form, unless it be sooner repressed or killed by exceptional meteorological conditions; and certain to become revitalized in each year of its epidemic existence in such situations of the occupied area as foster and preserve the germ during the period of dormancy.

During the last fifty years (since 1817) fresh emanations of cholera have proceeded from the endemic area every fourth, fifth, or sixth year, and can be traced over Northern India and the central provinces. During the earlier part of this period much of the country was not under British rule, and, in consequence, many gaps exist; still fixed landmarks may be found. In the later period, the history of each epidemic is complete. Take an abstract of the one for 1863–66, as an example. In 1863 (the first year) the cholera was confined to the eastern division of the epidemic area—the Agra district being the limit of invasion to the west. In 1864 (the second year) the cholera was general in the same area, both in the spring and monsoon reproduction, but making no advance to the northern provinces of the western division. In 1865 (the third year) cholera was still general in the eastern epidemic area, universally reproduced over the central provinces, advancing towards the north-west as far as Ferozepore, but only in the regions south of the Jumna. In 1866 (the fourth year) cholera, at a minimum in west of Behar, still existing in central provinces; in the northern provinces existing, but at a minimum. In 1867 (the fourth year in central provinces) extinct, and would have been extinct in the North-West Provinces (their fourth year also) but for a new invasion.

Every epidemic is a repetition of those which have gone before, and the parallels are as fixed as the recurrence of the meteorology of the invaded area. The maps show a singular uniformity with regard to the area attacked or free per annum during the outbreaks of 1854, 1858, 1862, and 1866; and the very dates of the outbreaks in the various stations correspond, commencing about the 20th June at Juggernauth (on the Bay of Bengal) to the 7th August at Lahore, following the monsoon and terminating with its influence, and *never* extending beyond the north-west desert, which is also the limit of the monsoon. As reproduction and invasion take place in coincidence with meteorological conditions, when these are not normal, the former may be *repressed* or *accelerated.* Thus, in certain years, the eastern epidemic area is assimilated to the western by the prolongation into it of the dry west winds. The eastern influence is then driven back, and reproduction of cholera is proportionally repressed; even invasions in progress may be checked when the governing influences fail, as in 1861, when the failure of the monsoon over the famine area repressed the cholera invading from the east and south. The high Punjab is not reached by the primary invasion of cholera, the summer rains not extending so far; but, with the spring rains and easterly winds of May, the districts beyond the monsoon influences are entered, the cholera being reproduced from that of the year before. Thus, when the frontier was attacked in 1858, 1862, and 1867, it was from a cholera reproduced from that of 1856, 1860, and 1866, which had extended over the east and south of the Punjab the year before. Mooltan is seldom reached by the monsoon, and, in consequence, is seldom affected by cholera in the same year as Lahore; but in 1856, when the weather was exceptional and rain extended from May to October, *Mooltan was reached* at same time as Lahore.

Although cholera requires moisture for its production or vitalization, yet that alone will not produce the disease. During 1867, in Nagpore, the rainfall was the heaviest noted for years, but there was no cholera. Cholera *may* be distributed in a dormant state, and there certainly is such a thing as the "aura" of an epidemic shown by manifestations of cholera in a harmless shape; which, however, are precursors of great and fatal outbreaks.

*Epidemic Progress.*—The essentials to this are the presence of cholera miasma, moist atmosphere as a vehicle, and a prevailing wind to give direction. Cholera *never moves along a highway.* The Grand Trunk Road is *the only one* from the eastern half of the Doab to the western, but when the western half of the epidemic area is invaded, Lahore is struck as soon as, or *even before*, *Umballa*, and the disease does not cross the Jhelum towards Peshawur and Cabool (the direction of the road). The geographical distribution of an invading cholera is a phenomenon of meteorological significance. The disease has never been spread over a geographical area by human intercourse *alone;* nor can human intercourse cause the boundaries of a natural province to be transgressed. The history of last year teaches that cholera carried by human intercourse beyond the bounds of its natural province is dead as an epidemic; and usual areas of exemption (and there are several such) continue free from year to year; and the most virulent epidemics are repelled, notwithstanding unlimited human intercourse by road and river with affected districts.

*Duration of Epidemics.*—An epidemic in any province is made up of the reproductions of a certain period, and extends over a series of years. The statistics of the last fifteen years appear to point to a term of four years as the regular one. The outbreak of 1817, invading from the south of the Jumna in 1818, survived until 1820, with a continuous history of reproduction; and it is probable that, if we could distinctly trace subsequent ones, the same period would be found holding good. With this outbreak, those of 1855, 1858, 1859, and 1862 are strictly parallel. Those of 1863 and 1866 make up between them a geography and history identical, but distributed over the like period of two epidemics. The epidemics between 1820 and 1855–58 also correspond; their histories have not been fully written out, but the statistical data have been kept in such a form as to render possible the construction of tables, connecting, in a clear cholera history, the years 1826 and 1868, and showing the aspects of cholera in relation to the soldier in former years contrasted with modern times.

After the commencement of an outbreak by invasion or revitalization, its intensity in different portions of the province often depends on the limitation of an advancing epidemic. Any natural circumstance which does this before the usual limits are reached causes a precipitation of the whole material, resulting in enormous strength, locally manifested either at the time or during revitalization. The Himalayan Mountains act as a limit of this sort; also a dry west wind, meeting with and checking the monsoon. Invading cholera occupies a province in a determined manner; often exploding, as it were, in many different portions of it at the same time, and its cessation is equally rapid; but in the area covered, all outbreaks are not synchronous with the invasion, but may occur at any time during its persistence. The termination is a natural phenomenon, and dates of both invasion and termination correspond in subsequent epidemics; and the geographical termination (Lahore) is reached on nearly the same day of the month in different years. All portions of a district, however, are not affected alike. In one the epidemic may be severe, in another it may be only a single case, or a strongly marked tendency to diarrhœa.

Invading cholera appears to come in a succession of waves. These may be, 1st, each wave a new body of invading cholera; 2d, the sprouting of cholera seed sown during the first wave; 3d, the renewal of the vehicle;—and all these must be considered in determining the aspect of the outbreaks of a province. The succession of waves is a very singular phenomenon at times. The interval is well marked by distinct termination of the disease; at others, it is only at a minimum; and instances are given in which there has not only been a second, but a third manifestation of this sort.

Dry open ground is not a breeding locality for cholera; but moist low lands along canal banks, the neighbourhood of jheels, and moist ground along the foot of the hills—all, in short, which we would class as malarious; and it is very rare for any station in the western area to be struck before rain falls.

Revitalization of cholera sown during an invasion usually takes place after a fortnight, and the revitalization in a province usually occurs after six months' dormancy.

There is no warning that an epidemic will proceed from the endemic area, unless it be an unusual prevalence of cholera within it, and the primary exit has (as above noted) no relation to lines of communication. In 1868, cholera, after prevailing in the western portions of Bengal, sent out an epidemic which spread between the margins of the endemic province of the Jubbulpore and Munda districts, covering a tract 500 miles wide, across a vast extent of country almost void of population, and without the usual channels of communication. It does not always follow the same route; after flowing over the high land of Huzareebaugh and Chota Nagpore, it may pass to the south and west (the southern route), or on to the Behar district (the north-west route), while in the eastern epidemic area the dates of expected invasion are uncertain, and even the least likely period (middle of December to middle of February) only gives a *minimum*, not security; in the western area the outbreak and reproduction may be expected to a day.

The difference between the infection of a community and a locality must be noted; the first may occur without the second, there being nothing to show that the cholera has found a habitat. Thus, a body of men moving through an infected locality may be infected, carry the cholera for a certain limited time, and become and continue free again; but a body of men remaining in an infected locality may have more than one attack. In Peshawur, in 1862, the 93d Regiment had four distinct outbreaks recognised as such by the surgeon of the regiment. In 1867, in the same place, the 42d was infected, marched out of the valley, and parted with the cholera in ten days; while the 77th, at the same station, which was forced by strategical reasons to remain in cantonments, suffered from the 21st May to the 25th June. The outbreak also varies in bodies of men of the same constitution (*e.g.*, Europeans), though subjected to the same choleraic influences. This occurred at Meerut, in 1819, when the 14th Regiment was attacked on the 8th August, and had 221 admissions and 41 deaths; at the same time, the Horse Artillery Brigade on the right, and the 8th Dragoons on the left, scarcely suffered. Again, in the same station, in 1867, the Buffs lost 138 (men, women, and children), yet there were but five cases in the Artillery, and two in the 19th Hussars.

*Duration of the Outbreak.—Normal.*—The outbreak *must* continue for a certain period, but *may* be greatly extended. The minimum is exemplified in a regiment struck on the line of march and escaping a second outbreak. The following table shows in the aggregate *every instance* in which a European force has been attacked on a march from 1856 onwards, and gives the admissions on each day, and the deaths on the admissions of each day:—

| Day, . . . . | 1, | 2, | 3, | 4, | 5, | 6, | 7, | 8, | 9, | 10, | 11, | 12, | 13, | 14, | 15, | 16, | 17, | 18, | 19, | 20. |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| Deaths on admission of the day, . . . | 35, | 32, | 27, | 28, | 9, | 7, | 6, | 9, | 11, | 5, | 6, | 10, | 5, | 2, | —, | 3, | 1, | —, | —, | 1. |

This gives 197 cases, of which 122, or 62 per cent. were admitted during the first four days, and 94 per cent. of all the fatal cases were admitted within the 12 days. A similar table, for native troops, shows that of them 98 per cent. of fatal cases were admitted within the same period (12 days). Tabulated results by Dr Lorimer of twenty regiments of the Madras army, struck by cholera on the march between the years 1829 and 1844, show that in 15 of the 20 cases the outbreak parted with its virulence on or before the 13th day, and in eighteen instances before the 17th day. The remaining 2 cases continued the virulence till the 20th and 22d day. On the average, it may be considered that a regiment on the march which is not re-attacked, parts with the virulence of the cholera before the 12th day; and it is very desirable that this, which is occasionally met with in outbreaks in stations, could always be secured.

Two instances of cholera on board ship adhering to the period are interesting. The "Renown" embarked the headquarter wing of H. M.'s 19th Regiment at Gibraltar, on the 21st August, cholera having appeared in the garrison on the 19th. Next day a fatal case occurred on board the ship. The vessel was hauled out in the bay, and detained thirty hours. No more cases occurring, she put to sea. On the 5th September, fourteen days from the fatal case at Gibraltar, cholera broke out and lasted fourteen days, carrying off the ship's surgeon, nine men, one woman, and several children. It was remarkable that this outbreak corresponded with the maximum of that at Gibraltar. The "Gertrude" left Calcutta on the 21st May 1859, and as cholera was very prevalent at the time, *every precaution* was used. On the 15th June, when in latitude 4° south, twenty-four days from all shore communication, and hundreds of miles from land, a soldier was taken ill and died next day, and four others (two soldiers and two of the ship's company) died between that date and the 25th.

In a cantonment which is itself infected, the duration of the outbreak is very different; thus, at Morar, during five years in which the cholera broke out, the outbreaks commenced between the 10th and 25th July, and concluded on 11th, 16th, 18th, 20th, and 27th September. At Meean Meer the commencement of the outbreaks is a month later, but the termination the same (8th, 15th, 16th, 22d, 28th September, and 1st October). The grand fact appears to be, that while the outbreak *may* be prolonged up to the period of natural decay of the epidemic, it cannot last beyond this; but we do not know why, while in the endemic provinces, and on the march, ten days sees the end of an outbreak in the epidemic area, it may be prolonged to six times that extent.

Any outbreak may have three phases: 1*st*, typical, confined within ten days from the date of poisoning; 2*d*, supplemented by the conditions of the locality, and enduring on to the death of the reproduction; and, 3*d*, an outbreak normal for the minimum, but continued, as to the body affected, to about the 16th or 17th day.

In cases of grand outbreaks, it is useless to suggest the possibility of infection from human emanations as the cause. Thus, we cannot suppose the grand army in 1817 was universally poisoned on the march, or that the latrines of five different barracks in Peshawur became simultaneously poisoned on the evening of the 19th May 1867. Nor in any instance in India has the outbreak been traced to polluted water-supply. Neither is the course of the disease that of one spreading by contagion: in the great outbreak in November 1817, the cholera *was dead on the 14th day.* The outbreak began on the 7th, and hundreds fell before evening. Kurrachee was struck on the 14th June 1846, and the same day 175 men were received into hospital from many different corps. Dum Dum was struck on the evening of 9th August 1859, and 17 fatal cases were admitted from different corps on the 10th. The great mortality is compressed within the first few days, and in no instance has the prolongation been coincident with the universality of the infection. As an instance of a body of men struck by cholera, written by officers without any theory, take Corbyn and Jameson's description of the great outbreak in 1817. Corbyn says: "I was the first person to observe it. On the 8th November a few dooly-bearers were brought to me, and, having never seen the disease before, I thought the men were in a state of inebriation, for the symptoms seemed to indicate as much; but soon discovering my mistake, I reported the circumstance, and was immediately directed to proceed to the encamping ground of the preceding day. The officer commanding the rear-guard having arrived, and reported that on the whole line of march multitudes were dead and dying, I proceeded, escorted by a strong guard of cavalry. Whole families, who in the course of the morning had accompanied their regiments, before proceeding many miles, were attacked by the disease, and I found them lying dead by the side of tanks and nullahs. On the following day, being the second of its appearance, in our camp the disease burst forth with indescribable violence in every direction."

Jameson continues the narrative: "Unsubjected to the laws of contact, this cholera outstripped the most fatal diseases hitherto known in the destructive rapidity of its progress. Previously to the 14th, it had overspread every part of the camp, sparing neither sex nor age in the undistinguishing virulence of its attack. The old and the young, the European and the native, fighting-men and camp-followers, equally sank in a few hours under its grasp. The mortality latterly became so great that there was neither time nor hands to carry off the bodies, which were thrown into the neighbouring ravines, or hastily committed to the earth on the spot in which they had expired. Hundreds dropped during each subsequent day's advance, and covered the roads with dead and dying; the ground of encampment and line of march presented the appearance of a field of battle, and of the track of an army retreating under every circumstance of discomfiture and distress. In this fatal week 764 fighting-men fell victims to the disorder, and of the camp-followers it was estimated that about 8000 were cut off."

Corresponding histories of cholera at Kurrachee in 1846, and at Peshawur in 1867, tell the same histories of an outbreak of intense virulence, the principal admissions and mortality being confined to the first few days. Were bodies always so struck, the cholera would probably die in a short time; but in many instances seed appears to be sown, which, like seed sown in a field, shall burst forth day by day, without intermission, until all that is destined to spring has sprung to life, as in the case of the 70th Regiment at Cawnpore in 1853, where the four deaths in May are to be considered the primary attack; on the 4th June appears the springing up of seed sown, the localizing cause being probably a cesspool, as the disease was confined to one barrack, about 20 yards from the south-west corner of which a cesspool had been recently opened, and the effluvium carried through the barrack. As soon as the monsoon set in (19th July), the disease sprang up again with renewed virulence; and on the 25th, a wing was removed into the Artillery Barracks, three miles distant, but continued to suffer. On the 4th and 5th August both wings marched into camp, six miles from the Artillery Barracks. The regiment parted with cholera on the seventh day after moving out (there having been 120 *admissions* after the 11th), and that although the cholera continued rife in the bazaars long after it had ceased in the regiment.

70th Regiment, Cawnpore, 1853. Strength, 911; admitted, 261; died, 183.

| | Deaths. | | Deaths. | | Deaths. |
|---|---|---|---|---|---|
| May 20 ... | 1 | * * | * | August 4 ... | 5 |
| „ 21 ... | 2 | July 19 ... | 2 | „ 5 ... | 4 |
| „ 24 ... | 1 | „ 21 ... | 2 | „ 6 ... | 7 |
| * * | * | „ 23 ... | 4 | „ 7 ... | 11 |
| June 4 ... | 5 | „ 24 ... | 8 | „ 8 ... | 8 |
| „ 5 ... | 5 | „ 25 ... | 13 | „ 9 ... | 6 |
| „ 6 ... | 1 | „ 26 ... | 7 | „ 10 ... | 3 |
| „ 7 ... | 6 | „ 27 ... | 13 | „ 11 ... | 1 |
| „ 8 ... | 4 | „ 28 ... | 4 | „ 14 ... | 1 |
| „ 9 ... | 2 | „ 29 ... | 8 | „ 16 ... | 1 |
| „ 11 ... | 1 | „ 30 ... | 8 | „ 17 ... | 1 |
| „ 15 ... | 1 | „ 31 ... | 6 | „ 24 ... | 1 |
| „ 19 ... | 2 | Aug. 1 ... | 13 | | — |
| „ 26 ... | 2 | „ 2 ... | 4 | Total, | 183 |
| „ 27 ... | 1 | „ 3 ... | 8 | | |

When special localities become infected, it is there that the attendants begin to suffer. It is admitted by the most zealous contagionist in India, that it is rare for attendants to suffer, and they hold the fact *inexplicable.* Fifty years' experience in the great Calcutta cholera hospitals has led a succession of careful observers to deny that infection from cholera patients ever takes place. Cholera becomes localized from certain causes. In Meerut cantonments the same bungalows and lines were affected in 1845, 1856, and 1861; the soil there is retentive clay, the open drains were flooded, and the compounds flooded, while the exempt portion of the station has a sandy and dry soil. Of course, an hospital may be affected as well as any barrack, as happened in 1861 in the 35th Regiment at Meerut, when cholera broke out in the hospital eight days before a case appeared in the barracks, but simultaneously with the occurrence of the disease in the barracks of the 8th Hussars; and in consequence of the *affection of the locality*, not only patients but attendants were seized. The localizing was here attributed to a faulty latrine at one corner of the hospital, and joined to it by a covered way. When any locality is infected, it is unsafe to use it for any purpose—most certainly not for the treatment of cholera. Many secondary causes tend to develop latent miasm (of cholera), and nothing more so than fatigue and anxiety attending service in a cholera hospital during an epidemic. In that of the 31st Regiment at Meean Meer, where comrades employed as orderlies were suffering seriously, natives were detailed to give help, but none suffered; and, at the same time, the exemption of the medical establishment was remarkable, and very few of the attendants suffered.

In a proper cholera hospital, not itself infected, there is less danger in attending the sick than in their own homes, and many valuable lives might have been saved had all cases been treated in general hospitals. The theory of the presence of the cholera germ in the evacuations would much facilitate the study of cholera, *could it be received as a truth.* This belief has been acted on in India, and precautions elaborated to the last degree, yet the liability of cantonments and regiments to cholera in its worst form is as great as at any time during the last fifty years, and the mortality is on the increase.

The intensity of the attack does not alone depend on the strength of the miasm. Stations which have been reputed healthy before being occupied by European troops, have afterwards frequently acquired the character of plague-stricken spots, examples of which may be given in Morar, Gwalior, and Meean Meer; but it is the presence of the European soldier himself which fixes the ratio of the attacks. Records show enough to make us aware, that if he had been there in previous outbreaks, these would have been serious.

In considering how to mitigate attacks, we have to put aside all minor facts and theories; and, *first*, that the cholera coming forward in an individual may be aborted. Granted; but this is not to be considered a combat with epidemic cholera. *Second*, that cholera may be averted by a system of prophylaxis. This proposition also has a basis of truth, but the general use of prophylactic means will not in very many instances prevent the outbreak, though lives, perhaps many, may be saved; for it is not every one who is cholera-poisoned who succumbs to the disease, but only where the constitution at the time favours the development. *Third*, quarantine. Allowed that this will be useful when we have evidence that the disease is being carried about by human intercourse, and, *if efficient*, will save life; but it has never been able to prevent the introduction of cholera into any country, and to trust to it to meet epidemics in India is to lean on a broken reed, and leave our soldiers virtually unprotected. *Fourth*, contagion. The aspect of outbreaks is strongly opposed to the idea that every developed case is a focus of contagion to all around; as, however, men may suffer from aggregation, and the cholera may select the tainted air over a sleeping mass, the idea must not be set aside; but it is an error to say that its ravages can be controlled by precautions directed against contagion.

If cholera could be prevented by perfect conservancy, and the use of untainted water, the dry-earth system recently introduced should have done so; but the events of 1867 give no great promise of fulfilling such hopes.

A high play of functional activity is necessary to maintain health in a cholera-bearing atmosphere. Predisposition to succumb is owing to many causes—the venereal taint, malaria, alcohol, overcrowding, or anything causing a temporary lowering of the system. Race has a most decided effect; Europeans, Ghoorkhas, Assamese, and frontier hill-men are those who suffer most—the natives of Hindostan almost escape; the outbreak with them being, as it were, a skeleton. The risk of attack in the European to the native soldier is as 10 to 1, and of death as 13 to 1. Good general sanitary conditions will not avert cholera. The sanitary state of the native soldier is very inferior to that of the European; their huts are small and ill ventilated, ground damp, water taken from tank, and not over pure, and his night duties are much more onerous. It is possible, however, that their great subdivision may have some effect, as, when massed together on board boats, the native can suffer severely. That this overcrowding affects the European was shown at Cawnpore in 1853, when the artillery escaped, while the 70th Regiment suffered; but on the artillery being doubled up to make more space for the regiment, the disease broke out and carried off twelve out of sixty men. As a precautional measure, tree-planting round and in our cantonments may be of service. It has been shown that in some places cholera adheres to the open plain, and scarcely attacks wooded parts; and in Chutteesghur, in the central provinces, never attacks travellers while passing through a wooded part of the district, while its ravages are fearful along a treeless portion of the road. A very important item in liability to cholera is "race." Englishmen, young and unacclimatised, or old and weakened, are always liable to suffer from climatic influence, and if exposed to cholera miasm suffer severely. Of this the Buffs at Meerut, in 1867, is a good example: in this regiment were 195 young men under 25; of these 53 died in their first year of Indian service (1867); there were 25 men above 35; of these 12 died the first year. Cholera came on this regiment in the end of August, when the depression caused by previous five months was at its maximum. In all probability the artillery and cavalry at the same station were exposed to the same degree of infection, yet of them, of a strength of 1040, only 5 men were lost, while of the Buffs 105 were carried off. Of the bad effects of alcohol, a striking instance occurred, when at a station (not named), of a troop of horse-artillery, 17 died of 18 carried to hospital, and of a cavalry regiment only 12 of 32; but the former were seized with panic, and took to drinking, so that many were carried drunk to hospital, while the cavalry regiment kept sober. The same treatment was pursued in both cases.

Many difficulties are confessedly left unanswered, and the grand desideratum is stated to be how to save European life—*if possible* to lower the European mortality to the native average, which would preserve one-fourth of all the lives now lost in India.

There is no appearance that the cholera miasm is falling into decay. When the North-West Provinces have escaped, it has been owing to chance diversions, and slight epidemics have been interpolated between severe ones; but comparative exemption will be found to be a provincial phenomenon compensated by severity in some other province of the epidemic area. The epidemic is not under human control. It is the outbreak and dependencies which are to be studied, and to which practical measures are to be applied. In India the cholera outbreaks have come to an epoch of severity so great that each epidemic appears to be worse than the previous. In 1818–21 the mortality was 25 per cent. of all attacked. In the sixteen years ending in 1854 the mortality was 41 per cent. of those attacked; but during the last ten years the ratio has never been below 50, while it has risen to 75 per cent.

Cholera, as above mentioned, does not depend on human agency distribution in time or space. The duration, reproduction, and distribution of an epidemic would be the same if the *locality were uninhabited*, and it might play out its part unobserved did it not meet with the human economy, and act upon it as a poison. It *may* be that cholera is multiplied in the human economy, buildings, latrines, and water, and may be transmitted by fomites; but it is no necessary sequence that cholera is never multiplied except in these ways, or geographically distributed except by human agency. The advance of cholera is not continuously progressive, as it would be were this the case. It is mapped out by successive occupation of natural provinces made *per saltum*, and at definite seasons. At any time between the epidemic leaps importation by fomites or human agency may occur in the countries beyond the already occupied area—but careful distinction must be made between importation and true epidemic advance, and it is a mistake to call the casual introduction of the *forerunners* of an epidemic the advance of epidemic cholera.

Where a system is built up from the secondary manifestations of cholera, the facts alleged may be true or otherwise, but they will be found leaning on one another for support, having been added in each succeeding European outbreak, as it was found that the prop on which the previous theory rested had been struck out. Thus, the facts of the first European invasion showed that aerial transmission did not account for all the phenomena observed, as transmission by fomites and occasional infection by attendants. Hence was initiated (*a*) the doctrine that cholera might be propagated by human intercourse; and at the latest phase of this doctrine we find the confident assertion promulgated as a truth, that cholera is *always*, and not occasionally, so propagated. To prop this assertion it was necessary to make a second assumption or theory, and hence arose the doctrine (*b*) that cholera is multiplied in the human economy. But this also must have stood alone and unsupported, unless it could be shown how and where the multiplication took place; and this led to the starting of the third theory (*c*), which asserts that cholera is multiplied in, and spread around by, the intestinal evacuations of those already suffering from the disease. But even this, though urged in the most forcible manner, did not meet all difficulties, and then arose the demand that it should be supplimented by a fourth theory. In relation to this demand, the last theory (*d*) alleges that the evacuations of an individual in whom cholera has not become apparent, and never will appear, may be the means of spreading cholera around. But even supposing that the truths on which the above theories rest were true, even to the full extent asserted, they *cannot be made the basis of a substantial and complete system which shall supersede every other.*

The value of the primary and secondary truths is probably different in India and Europe. There is no doubt that the pilgrim stream from Hurdwar in 1867 was cholera-tainted, and had the power to give it off; and the exemption of the city of Jullundur, while a small portion of its inhabitants, sent out to turn off the pilgrim stream and supply their wants, suffered, shows that pilgrim streams may be sources of infection, and that by using means cities may escape. But it is certain that there is a cholera, against which quarantine is unavailing. But even when cholera appears to have been introduced by human intercourse the circumstances are not always convincing, for no pilgrim body will develop cholera, except when that disease exists and is abroad; and every manifestation which occurs has its *place in the epidemic which is in progress.* It is undoubted that the disease may be transmitted by fomites, may be spread by fomites; it is, however, the clothing which is offensive to the sense of smell which transmits cholera. Pilgrims are said to be a terror to the native population, and the villagers can recognise them by *smell!* and it would appear from the cases of the vessels "Renown" and "Gertrude" that the fomites may remain active for three weeks or twenty-five days. The sum of our experience appears to be that, while the aggregate of outbreaks of soil-born cholera through successive reproductions makes up an epidemic, there is no evidence to show (in Bengal) that any aggregate of cases, derived secondarily from true epidemics by human agency, has ever given rise to an epidemic. Cholera in rural districts is not of shorter period than in cities. The reproduction is the same in all classes of the population.

Contrast Baly's views (Report, p. 216–224) with those of the Constantinople Conference. The first, in a condensed form, are—

1. It is not proved that cholera is spread subsequent to its multiplication in the human economy.

2. Assumes the controlling power of atmospheric and hygrometric influences.

3. Holds that cholera is not contagious in the sense in which we apply the term to the method of operation of other zymotic poisons, and that the theory of spreading by contagion is opposed to conclusions drawn from well-observed facts, which place cholera in a different category.

4. Assigns to human agency a share in dissemination of cholera. but—

5. The theory of provincial distribution of cholera by a controlling aerial agency is entirely in accordance with the Report.

"It is certain that the extension of the disease over large towns, if not over larger areas, may take place independently of communication between the sick and the healthy. In cases where human agency cannot have been the means of diffusing cholera, the agent most likely to have conveyed the poison from one spot to another is the wind."

6. Cholera is recognised as a material substance not of a gaseous nature, the area of the distribution of which is marked by the presence of the object cholera. Relation to a vehicle of moisture is also hinted at.

7. Baly recognises that, beyond all other conceptions of its relations, epidemic cholera presents certain phenomena which are inexplicable, and finally, for want of evidence, he leaves open the question of poisoning by emanations from the sick, and of methods of dissemination other than those indicated.

The conclusions of the Constantinople Conference are—

1. No fact has hitherto been brought forward to prove that cholera is spread abroad by the atmosphere alone, whatever may be its condition.

2. The principle of cholera is regenerated in man, and accompanies him in all his peregrinations; it may thus be spread far and wide from country to country by successive regenerations, never reproducing itself spontaneously, except in the human frame.

3. The matter of choleraic dejections is incontestably the principal receptacle of the morbific agent. It follows, therefore, that everything contaminated by such dejections also becomes a receptacle, whence the generating principle of cholera is evolved under the influence of favourable conditions. It also follows that the choleraic germ very probably has its origin in the digestive canal, to the exclusion, perhaps, of every other part of the organism.

4. Extension of cholera has always taken place in the direction of the human currents which have set out from the place where it was raging. Cholera has never in its progress preferentially taken, as believed by some, a direction from east to west, but, on the contrary, it has radiated, and radiates, in India in every direction, to the south as to the north, to the east as to the west, spreading itself everywhere in consequence of the facility and multiplicity of the communications. Those who think otherwise have not studied the facts, and reason as the Chinese would do, who pretend that cholera invariably proceeds from west to east.

5. The study of facts demonstrates that there is no proportion between the amount of cholera imported and the intensity of the epidemic consequent upon it, as a conflagration is not proportionate to the spark which gave rise to it. Man tainted with cholera is the

principal propagating agent of the disease, and a single case of cholera may give rise to an epidemic.

These last views are totally opposed to Indian experience. Jameson in 1819, Corbyn in 1832, M'Kinnon in 1848, and Scriven in 1863, all consider that the *usual* method of propagation cannot be by contagion, giving, among many other reasons, the almost total exemption of medical and hospital attendants; and the rise, rapid diffusion, and speedy cessation of the disease, and that in the absence of any effective precautions against it. The apparent antagonism of the facts regarding cholera is in most cases to be attributed to the want of intelligent appreciation of their significance, and the less the whole truth is grasped, the stronger appears all that is put down to the inexplicable. If there be any truth in what has here been deduced from facts, there is no room for extreme views, especially for such as have most recently been put forth upon authority.

Such is, we consider, a fair abstract of Dr Bryden's views; and we must confess that, having seen not a little of cholera both in India and this country, we are inclined to accept at least the greater portion of them; and even where we withhold full acquiescence, it is chiefly because we wish to know more, than that we have grave doubts of the theory. We think that in this country disease is more frequently spread by fomites and by human intercourse than in India. Washing soiled clothing, strange to say, appears a common cause of infection here, while the Indian washermen *almost never* suffer, and they wash, or used to wash, every rag—nothing was destroyed by fire as here. The localization of the disease, also, by defective drainage, and specially by polluted water, is certainly common here; and, as a contrast, the effect of improving drainage and removing nuisances can be shown to have been very effectual in protecting a village notorious for having suffered in every outbreak of cholera which reached Scotland until the last.

We have also seen cholera refusing to be localized in places with good water-supply (even though the drainage might be indifferent), though carried there by persons fleeing from suffering localities; and we think we could find many instances of the twelve to fourteen day theory, with the heavy mortality confined to the first few days of the outbreak.

In conclusion, we would only remark, that if cholera always repeats itself (and there is much to prove that it does), we should be preparing for it. It is reported as having passed from India both to Africa and Russia, and we may expect a visit at no long-delayed date. It would surely be wise to look to all our sanitary arrangements, especially our drainage and water-supply, so that the epidemic, when it does come, may meet nothing to localize it, but may find us with our house swept and in order.

*Die vier Factoren der Geburt.* Von DR WILH. L. KUENEKE, Director der Entbindungs- und Hebammenlehranstalt zu Celle. Berlin: 1869.

*The Four Factors of Labour.* By Dr W. L. KUENEKE, Director of the Lying-in and Midwives' Training Instititution at Celle. Berlin: 1869.

ALTHOUGH this work of Dr Kueneke's is not very well or appropriately named, it is nevertheless a highly creditable production, which will command the perusal of those interested in the progress of the science of midwifery. It is really a treatise on the mechanism of labour, and its division of the main elements of the process into four factors is, in our opinion, not a fortunate one.

When we say that the work is on the mechanism of labour, we do not mean that it is confined to the consideration of that corner of the great subject which generally gets, *par excellence*, that designation. It includes it. The greatest progresses in all the natural sciences consist at present in the discovery and elucidation of mechanism. Much more is "mechanism" than was formerly supposed; much indeed of what was or is supposed to be vital. Who can read Schwartz's work on Premature Breath-movements without seeing that almost all that is a chapter of the mechanism of delivery? Yet though Kueneke extends the scope of "mechanism," he does not include the subjects discussed in Schwartz's excellent volume.

Kueneke's first factor is "the mechanical proceedings of labour." It includes the attitude and position of the fœtus, changes in position; mechanism of vertex cases, describing a synclitic movement and cyrtophoric progression, a peristrophic or turbinal or rotative movement, an enclitic movement, a proclitic movement, and a cataclitic movement. Some of these movements are newly described, and we fear, in some points, not truly so. There can be no doubt as to the novelty of the nomenclature. Kueneke makes sad havoc on Naegele's mechanism, and to a considerable extent with much justice.

The second factor is "the organic proceedings of labour." This includes the cause of labour, the changes in the uterus and cervix, in the floor of the pelvis, the perineum, and other topics.

The third factor is "the dynamical proceedings of labour." It enters on the pains and the assistant expulsive powers of labour.

The fourth factor is "the plastic proceedings of labour." It describes the moulding of the child's head under different circumstances.

We shall not enter on any criticism of the work, satisfying ourselves with calling the attention of obstetricians to it, and assuring them that they will find it highly deserving of perusal and study. It contains much that is novel.

*On Food: Its Varieties, Chemical Composition, Nutritive Value, Comparative Digestibility, Physiological Functions and Uses, Preparations, Culinary Treatment, Preservation, Adulteration, etc.* By H. LETHEBY, M.B., M.A., Ph.D., etc. London: Longmans, Green & Co. Pp. xiii. 277.

INQUIRIES into the origin of life, and its physical basis, have been of late attracting not a little attention. And no wonder; for very fascinating to minds of a certain order is the mystery in which these subjects are enwrapped. And yet, so far as we can at present discover, the hypotheses which may be formed regarding them are, and will be, barren of practical advantage. Not so, however, with an allied subject—the means of sustaining, and of developing life. This subject may be deficient in the obscurity which attracts so powerfully towards the more recondite researches which the other questions demand, but it is difficult to overestimate its importance, especially to members of our profession. We hope the time is rapidly approaching when the physician *shall* be as conversant with the qualities of the articles of food as he is with the properties of drugs, and shall be as attentive to the *nature of*, and most suitable modes of, preparing the diet of his patients, as he is to the effects of the medicines he exhibits. Had the regular practitioner been more attentive to this very important branch of his profession, there would have been a much narrower field for the outsider and the quack.

The work before us consists (of the substance) of four (Cantor) Lectures, delivered two years ago before the Society for the Encouragement of Arts, Manufactures, and Commerce. They were published at the time in several English and American journals; and as the author found that American and French editions of them were in course of publication, he was induced to revise and amend them, and to recalculate the tables, so that they are in accordance with the most approved analytical data.

The importance of having in a popular form the results of the investigations of the many distinguished men, British and foreign, who have made the subject of Food, with its many allied topics, the object of their special research, will be readily perceived, and Dr Letheby's character and attainments afford a sufficient guarantee that the work has been ably and accurately performed. The highest expectations on this point will not be disappointed.

The first lecture is devoted to the *Varieties of Food; their Chemical Composition and Nutritive Value.* Casting a rapid glance over the vast array of substances, vegetable and animal, solid and fluid, employed as food, he exhibits generally, in a tabular form, their Constituents, Nutritive Value, and Comparative Costliness, as means of supporting life, and imparting ability to labour. Thus an immense amount of information, at once important and interesting, is placed in a compendious form within our reach.

The second lecture treats of the *Comparative Digestibility of Foods, and the Functions of different Foods.* Under the latter of these heads, he gives a brief but clear account of the recent change of scientific opinion regarding the respective functions of the two great varieties into which foods are classified. On this point he does not dogmatize. He says, "The functions of the several constituents of food have been examined from new points of view; and the opinion of Liebig, as to the special force-producing or motive power of nitrogenous food, have been carefully considered. This has become necessary from the circumstance that they have been recently tested by actual experiment, and it has been found that the muscular energies of the body may be sustained, and considerable labour performed with a purely non-nitrogenous diet. It is even probable that the thermotic power of food is at all times the true exponent of its mechanical or motive power, and that this is quite independent of its nitrogenous or non-nitrogenous nature; although it is evident, from chemical considerations, that the carbo-hydrogens, as fat, sugar, and farinaceous matters, are far more sustaining, in a thermotic sense, than nitrogenous foods, whose functions are perhaps chiefly to repair the waste of tissues. It must be admitted, however, that this part of the subject is still incomplete, and requires much experimental investigation before it can be satisfactorily determined."

In lecture third are considered *the Constitution of Dietaries, and the Preparation and Culinary Treatment of Foods.* The subject of dietaries is one of such importance that it may be looked upon as a national question. The pauper class are too often *under-fed*, and the results are that both the bodies and minds of a large section of our fellow-countrymen are so debilitated that the power and the will to recover themselves and to raise their offspring from the abyss into which they have sunk is in danger of being irretrievably lost. Justice and mercy alike demand that this evil be remedied. On the other hand, the prison dietary as often errs in excess. Until the early part of last century the treatment of prisoners was a disgrace to humanity. John Howard had the honour of directing the European mind to the crying evils of the system. Thanks to the self-denying zeal and efforts of philanthropists, chiefly of the Quaker community, the reform was carried on with such success, that it may be suspected that the pendulum has need of a movement in the opposite direction. The criminal classes are frequently so well fed in prison that they become turbulent and mutinous; nor is this the whole of the evil,—they have not the salutary dread of the jail which a place of punishment ought to inspire. Nor can any severity of treatment which the public mind could be induced to sanction, nor any amount of labour which the criminals could be persuaded to perform, avail to render the prison life so distasteful as it ought to be. With the sensual and the degraded, the appetites have an overwhelming influence in determining actions. The cruelty and countless woes of "the house of bondage" lost their terrors to the Israelites when they thought of the fish of Egypt and the savoury herbs. The prison dietaries should be formed on a comprehensive plan. They should be uniform—not capricious, as they are at present in too many cases. Justice and the truest mercy demand from society the repression of this pseudo-philanthropy, which has too long been allowed to sway the public mind in this matter. No cruelty, no starvation is needful, but a just regard to the wants of the human frame, and a due estimate of the most efficacious means of reforming our criminal and dangerous classes. This has become a most important social and moral problem.

In the concluding lecture the subject of the *Preservation of Food* is efficiently dealt with. The history and general principles are given, and the various processes are described. The regulations respecting unsound and diseased meat—the signs of good and of bad meat—the effect upon the system of meat infected with disease, as measles and trichina, are explained; and, lastly, the lecturer finishes with a description of the various adulterations which are practised in regard to food.

We close our notice with the expression of our high approbation of Dr Letheby's treatise. Seldom is such an extensive amount of knowledge presented in such an interesting manner. We everywhere see the hand of one who is perfectly familiar with his subject, wide-ranging though it be. We cordially commend the work to the favourable notice of our readers.

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*Faraday as a Discoverer.* By JOHN TYNDALL. New Edition. London: 1870.

*The Life and Letters of Faraday.* By DR BENCE JONES, Secretary of the Royal Institution. Two Vols. London: 1870.

THE daily life of scientific and learned men is in general easily separable from the work which gives them reputation. Their daily life is regular, and devoid of adventure, while their work is abstruse, and often uninteresting to the world. The only striking passage in the life of Michael Faraday is his escape from a condition of life where the cravings for study and research are in great danger of being subdued by the hard work necessary to gain bread to a position where every facility was in his hands for the prosecution of his laborious scientific researches. He was born in 1791, the son of a blacksmith, and worked as bookbinder in a shop in Blandford Street, London. Devoting his spare time to the study of Natural Philosophy and Chemistry, he succeeded in attracting the notice of Sir Humphry Davy, who gave him employment in the Laboratory of the Royal Institution, at first in a humble capacity, which a man with better antecedents would have refused, but which gave him the training and opportunities of improvement which he needed, and finally put into his hands means and apparatus of research the wealth of a Cavendish could not have enabled him to surpass. In 1812, when Davy, at the invitation of Napoleon, went to Paris to receive the prize awarded to him for the decomposition of potassium and sodium, he took Faraday with him as his assistant and amanuensis. Faraday spent a year and a half with his patron in France and Italy, holding converse with many distinguished scientific men. On returning, he resumed his old situation at the Royal Institution. He commenced original researches, and slowly rose to eminence as their results became known. In 1825 he was appointed Director of the Laboratory of the Royal Institution, and afterwards Lecturer of the Royal Academy at Woolwich.

We never knew a man who liked anybody to get on better, and few who like another to get on as well as himself; and Faraday had the mortification of finding his old benefactor and friend, Sir Humphry Davy, actively oppose his admission to the Royal Society, which took place in 1824. This was the only honour Faraday sought. They came unsought from all countries in the civilized world, the homage of philosophers everywhere paid to that *vivida vis animai* which enabled him to solve so many subtle questions in physical science. Faraday retained to the last the simplicity of his character; he preferred mental liberty—the liberty of studying and interrogating nature—to all the riches and noisy honours which he could easily have obtained, by doing the scientific work which was best paid, and therefore most honoured by the world. A character so singular may well claim two biographers.

The work of Dr Bence Jones is more especially devoted to Faraday's private life, though not exclusively so. We have several diaries and notes of travel, and a large number of letters written to and from the most eminent of Faraday's fellow-labourers in Natural Philosophy and Chemistry. Some of these are of great interest and importance, often beautifully written, and possessing great interest both to those who wish to follow Faraday in making original researches, and to those who are constrained to pay a more distant court to nature. Nevertheless, many of these letters to and from Faraday are trivial; some of them mere letters of introduction, others dealing with questions now hopelessly buried under the drift of the scientific gossip of years, taking pages to record events which might perhaps claim a sentence. Dr Bence Jones's book could be much improved by being reduced one-half; as it is, it is more materials for a life of Faraday than a life itself. Nevertheless, with a little more refining, and a little more time and labour, it might become a good biography. At present, we can only say, that it contains much interesting matter and some good writing. This is an error which a warm friend turned biographer very often falls into. Dr Bence Jones is well known to be an able chemist, and no doubt could, if he tried, separate the valuable part of these volumes, as well as he could extract so much quinine from so much cinchona-bark.

The little work of Tyndall, the worthy successor of Davy and Faraday at the Royal Institution, deals more with the discoveries, and less with the daily life, of Faraday. Tyndall gives us his method of investigating nature, so far as such processes of thought are capable of being described. The style of the little book is singularly happy, close, powerful, and suggestive. Now and then it might be clearer; but the aim of the work is thoroughly realized. He who reads and understands it has in his mind a portrait of the thoughtful sage, which will not readily fade away, and which he will contemplate with the deepest veneration.

It is likely that Faraday found the want of an early training in mathematics; and in his letters he more than once regrets his ignorance of German; otherwise his training was most complete. He used to say that it took twenty years' work to make a man in physical science. In the infancy of chemistry Priestley, who never worked very hard at his laboratory, discovered oxygen; but the fruits of the discovery were carried off by Lavoisier, "in a series of researches, strongly bound together by one dominant idea, and rendered immortal by skill in experiment, clearness of exposition, and rigorous exactness of deduction,"[1]—words which apply to Faraday even with greater force. The leading generalization which the English philosopher pursued in his long and busy life, and sought to verify by experiments, was that all the forces of matter, chemical affinity, electricity, magnetism, heat, and gravitation, were related to one another; that, to use his own expression, "the various forms under which the forces of matter are made manifest have one common origin, or, in other words, are so directly related and mutually dependent, that they are convertible, as it were, into one another, and possess equivalents of power in their action."[2]

In 1831, he succeeded in evolving electricity from magnetism, as magnetism had been previously evolved from electricity. In 1845 he experimentally showed the connexion of light with magnetism, but he never succeeded in showing the connexion of gravitation with heat or electricity. Working from studious deductions in a truly inductive manner, he was a great theorist, but a slow and cautious theorizer, and matchless experimentalist. Applying his powerful and subtle intellect upon one subject at a time, Faraday was wary of reasoning from experiments which he had not himself repeated. Following out a grand generalization, but with great powers of lateral vision, he tested his results in a severe and

[1] A History of Chemical Theory, by Ad. Wurtz, 1869, p. 11.

[2] Dr Bence Jones, vol. ii. p. 199.

coldly critical manner, subjecting them again and again to the test and cross-examination of new experiments. Every discovery which he announced was made in a thorough and exhaustive manner. He solved questions which baffled the sagacity of men like Arago, Ampère, and Herschel, in a way which left little for the objections and improvements, though always a wide horizon beyond for the labour of others.

We have no space to detail the many other important contributions which Faraday gave to physics and chemistry. Any addition to these sciences is an approach to a scientific basis for medicine, which can only be hoped for from an explanation of all the subtle forces which enter into the production of vital phenomena. One need not therefore ask, What is the use of Faraday's great discoveries? The more they are applied and extended, the more useful they will appear. We may retort with Franklin, What is the use of an infant? Very little when he is first born, but useful when he grows up; so the longer Faraday's discoveries are studied and applied, the more their use will appear.

Owing to the intense strain of his mental work, Faraday became troubled with giddiness and loss of memory. For a year (1841) he was obliged to suspend all work whatever, nor did he ever entirely recover his memory or capacity for mental labour. During the last few years of his life, which closed in 1867, he was unable to carry on any researches.

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## *Lunacy: its Past and its Present.* By ROBERT GARDINER HILL, F.S.A. London: Longmans, Green, Reader, and Dyer: 1870.

IN the book now before us, Mr Gardiner Hill puts forward most forcibly his claim to be considered the father of the non-restraint system of management of lunatics. *Aut Cæsar, aut nullus* is his motto: he claims for himself all credit and praise due for the enunciation of the great principle. Whatever the cause may have been, there is no doubt that Hill has never received from the world at large that full acknowledgment of his services which is his due; the name of Conolly seems to have absorbed the large proportion of credit, for with it is generally associated the carrying out of a principle which in fact he had learned from Hill. Curiously enough, this does not disturb the author's equanimity so much as that Dr Charlesworth's name has been associated intimately with his own in the development of the principle of non-restraint.

What Hill did was to assert positively his conviction, and to act on it, that restraint could be dispensed with *altogether*. It was a great thing to do, and all honour is due to him, for having done it. To him belongs the credit of having placed the capital on the pillar, the foundation-stone of which, however, had been laid long before he was born.

On calm reflection, a feeling of wonder arises that the consummation of the work was so long delayed. Forty-six years previous to the total abolition of restraint in the Lincoln Asylum, the Quaker, William Tuke of York, established the humane system of treatment of lunatics in the Retreat; he exposed the great, grievous, crying abuses of asylums, and came forward as the Howard of the insane. In the same year, Pinel struck off the chains from the limbs of the inmates of the Bicêtre, and enacted those scenes, the vivid description of which must live for ever in the minds of all true philanthropists. It is, possibly, a feeling of individual wrong which has prevented Mr Hill in his book from acknowledging more fully and gratefully the services of these men. The abolition of restraint was not a heaven-born impulse in him, it was the result of education—for it took three years to demonstrate the fact to him that the maniac could go unchained. The main idea originated with Tuke and Pinel. It was the former who first brought to bear on the treatment of the insane the true non-restraint principles of work, exercise, association, and mutual confidence. It is true that mechanical appliances were occasionally used in the Retreat, but they were of the mildest possible description; and at the time when Mr Hill had arrived at the years of manhood, they had been reduced to a minimum, to a degree not much greater, if any, than exists in most asylums at the present day. In the years during which Mr Hill had 19 and 10 per cent. of his patients restrained, the amount of mechanical appliances in the Retreat at York was much less. In 1813, this asylum was nearly up to the mark, if not quite so, of those of the present day, the only means of restraint in use being a light *camisole* and a belt round the waist slightly confining the hands. For more than thirty years after, we read of manacles, hand-cuffs, chains, leg-locks, and quarter-boots, as being in constant use in the English county asylums. The Committee of the House of Commons which sat in 1815 mitigated to some extent this miserable state of matters, but failed to bring asylum management and treatment up to the level of that of the York Retreat. It would seem that the exclusiveness of the sect to which William and Samuel Tuke belonged prevented their system from being more widely known. Still it was known, and why it was not sooner acted on it is difficult to say.

Dr Charlesworth, of the Lincoln Asylum, paid much attention to the improvement of the form and extent of mechanical restraint, and its reduction was, as Dr Hill himself said in his Lecture on the Management of Lunatic Asylums (1839), "pressed upon the attention of the boards of the Lincoln Asylum by its able and humane physician, Dr Charlesworth, at whose suggestion many of the more cruel instruments of restraint were long since destroyed, very many valuable improvements and facilities gradually adopted, and machinery set in motion, *which has led* to the unhoped-for result of actual abolition, under a firm determination to work out the system to its utmost possible limits" (the italics are our own). From this quotation it is not difficult to trace out the course of education which Mr Hill had undergone. He watched thoughtfully the improvements of Dr Charlesworth, he had the example of the Retreat before him, and at last, by a happy thought, or succession of thoughts, came to the conclusion that restraint might be entirely abolished.

Conolly learnt from him, and the pupil outstepped the master in the race of life. Conolly had the happy knack of keeping himself before the public; he worked out the non-restraint system at Hanwell, and let London know that he had done so, and London in those days was the world.

To Hill is due the credit of doing away with shackles and strait-jackets; to William and Samuel Tuke, of York, is due the enunciation of the leading principles on which is based our present humane system of treatment. Their names, and that of Pinel, must ever stand as the prime movers. They established, by word and by work, that practice which Hill eventually was happily the means of elaborating. He cannot be regarded as the *parturiens mons*, nor can he be admitted to be the father of the child; his position is that of a thoughtful midwife, who brought to light a full-grown healthy infant, whose term of utero-gestation had been prolonged for forty-six years, and which was delivered to the world after a difficult and dangerous labour. William Tuke was the father.

# Part Third.

## MEETINGS OF SOCIETIES.

### MEDICO-CHIRURGICAL SOCIETY OF EDINBURGH.

### SESSION XLIX.—MEETING VII.

*Wednesday*, 16*th March* 1870.—Dr BENNETT, *President*, in the Chair.

I. *Mr Annandale* showed a patient who had been four times operated upon for EPITHELIAL CANCER before coming under his care. Finding that the disease affected the alveolar margin of the jaw, as well as the lip and cheek, Mr Annandale removed very freely the soft textures implicated, and also sawed off two inches of the alveolar portion of the jaw. It was six months since the operation, and the patient remained perfectly free from any return of the disease.

II. THREE CASTS to illustrate the amount of cut surface in different amputations at the hip-joint.

III. A TUMOUR OF THE LOWER JAW, for which he had removed one half of the jaw. The stitches were removed on the fifth day, and the patient made a good recovery.

IV. A GLANDULAR TUMOUR, which he had removed from the breast of a young lady. The case was interesting from the fact, that there was a second tumour separate from but adherent to the first one.

V. A MEDULLARY TUMOUR, the size of a walnut, which he had removed from the hand of a patient.

VI. TWO CONTRACTED TOES, which he had removed from two patients. The condition of the toes was such as to prevent walking, and cause the patients to ask for the removal of the digits.

VII. A case of COMPOUND COMMINUTED FRACTURE of the lower end of the humerus, for which he had performed primary excision of the elbow-joint. The interest of the case was that the patient, who had suffered from fits when a boy, was seized two days after the operation with a severe epileptic fit, which proved fatal in less than twenty-four hours.

VIII. *Dr Watson* showed THE BASE OF THE INFERIOR MAXILLA, to which was attached a V-shaped portion of the whole thickness of the soft parts covering the incisor and canine regions of the bone, removed from a man aged 69. The disease was epithelioma, originating in the gum covering the alveolar process of the bone in the incisor region. The disease had existed for some years, extruding the teeth, and had extended as far as the molar region on each side, invading also the floor of the mouth and frænum of the tongue. The operation was performed by making a horizontal incision below and parallel to the line of the prolabium, and extending this through the cheek on either side as far as the angles of the bone, and then by carrying an incision on each side downwards and inwards from the bicuspid region of this transverse cut, so as to meet one inch below the symphysis. The incisions thus effected left a V-shaped portion of the soft parts covering the mental region. The lateral flaps were then dissected up from the base and angles of the jaw, the bone divided at the angle of the jaw on each side, and with a sweep of the knife the soft parts in front of the tongue were cut through. The patient has made a satisfactory recovery.

IX. A LARGE FIBROUS TUMOUR removed from the parotid region of an old man. The only interesting feature in the case was the existence of paralysis of the portio dura on the affected side for more than a year, a symptom which had apparently deterred some eminent surgical authorities whom he had consulted from recommending operative interference.

X. AN EPULIS from the right side of the superior maxilla of a child, involving the bicuspid and molar regions, removed by a horizontal incision by the saw along the base of the alveolar process, and completing the detachment by means of the bone pliers.

XI. AN EPITHELIOMA FROM THE TIP OF THE TONGUE, of the size of a sixpence, perfectly limited, and without glandular implication. The removal was effected by the knife.

XII. THE ENTIRE RADIUS, exclusive of the epiphyses, removed as a sequestrum from a boy of eight years of age. The disease had existed for nine months.

XIII. *Mr Benjamin Bell* then read his paper ON A PECULIAR PARALYTIC CONDITION OF THE LOWER EXTREMITIES FOLLOWING GASTRIC FEVER, which appears in the present number of this Journal, at p. 978. As the immediately following paper was on a closely allied subject, it was agreed to read it first, and to discuss both papers at once.

XIV. *Dr Grainger Stewart* then read his paper, entitled' TWO CASES OF DIPTHERITIC PARALYSIS SIMULATING LOCOMOTOR ATAXY, which appears in our present number, p. 988.

*Dr Bennett* remarked on the great importance of these two papers, indicating as they did the occurrence of important lesions of the spinal cord following two different epidemic diseases; he himself was not aware of having seen any similar cases, and he rather thought that none such had been recorded. Ataxia was a disease which had been only recently discriminated from the other forms of paraplegia; it consisted essentially in a defective power of performing combined movements, while there was no want of muscular power; commonly, cutaneous sensibility was diminished, but occasionally it was increased; its symptoms differed entirely from those of muscular paralysis, and from those of progressive muscular atrophy, the muscles in locomotor ataxy keeping always well nourished. The pathology also differed entirely; in paraplegia the white matter of the cord was diseased, but in ataxy, according to Dr Lockhart Clarke, the gray matter was the seat of lesion, this lesion being a fine molecular degeneration, which fully accounted for the symptoms. He observed that strychnine had been employed in the treatment of these affections; he certainly should not expect it to be beneficial in such cases, and indeed he had never found it to be useful in any organic cases of paraplegia whatever.

*Mr Annandale* remarked that he had now under his care in the Royal Infirmary a patient aged 30, who, in falling, had struck her head against a stone step, and remained insensible for twenty or thirty minutes. The results were considerable pain in the upper part of the back and over the shoulder-joint, diffuse swelling of the neck, and cutaneous insensibility of that part of the scalp to which the great occipital nerve was distributed. The case was interesting in regard to the question as to where the seat of lesion was: Was it in the upper part of the cord, or simply confined to the great occipital nerve as it passed over the atlas? His own opinion was, that it was confined to the nerve.

*Dr George W. Balfour* stated that he perfectly agreed with Dr Bennett in regard to the inutility of strychnine in cases of locomotor ataxy; at the same time he could not agree in the opinion pronounced against it in all cases of paraplegia. If there was one disease more than another in which careful diagnosis and discrimination of the exact nature of the lesion previous to selecting a remedy was necessary, that disease was paraplegia. Whenever there was any evidence of defective nutrition of the cord, then strychnine was pre-eminently useful; this was specially the case in reflex paralysis; and he had several very interesting cases of this kind, in which strychnine had apparently been extremely useful. One of the most remarkable of these was a case of subacute nephritis, in which reflex paralysis of the bladder, rectum, and lower limbs occurred, and in it a perfect cure had apparently been principally promoted by the use of strychnine; and this theoretical view was supported by a similar result in many similar cases. On the other hand, in congestive diseases of the cord, strychnine was extremely injurious, and constantly aggravated all the symptoms until after the congestion had been subdued, when it took its place as a tonic, and assisted materially in improving the condition of the cord, which had been impaired by the long-continued congestion. He had just discharged a patient as convalescent, and now able to walk with almost normal firmness, who, seven months ago, could not stand, and whose congestive myelitic symptoms had to be treated for many months with ergot before he could bear without aggravation the stimulating action of the strychnine. At present he had in his wards a patient whose congestive attack dated five months back, and in whom he therefore thought the vascular system of the cord would be sufficiently quiescent to permit the employment of strychnine; but the result was unfavourable, the symptoms were aggravated, and had to be—as they were—relieved by a recourse to ergot. Without at present entering further upon the treatment most useful in the various forms of paraplegia, he would merely remark that he agreed with Dr Stewart in regarding the nitrate of silver as almost a specific for locomotor ataxy. In his own experience, nitrate of silver and Faradization had proved invariably successful in ameliorating the symptoms, and was occasionally successful in arresting the progress of that disease; in one case a perfect cure had resulted,—the patient had been relieved from all his ataxic symptoms, had been enabled to follow his ordinary employment, and had finally died from a different disease—cerebral hæmorrhage.

*Dr Argyll Robertson* remarked, that Dr Stewart had omitted one very important point of the differential diagnosis between true locomotor ataxy and that following diphtheria, which was, that in the former spinal myosis was almost invariably—or at least very frequently—present; while in the latter form of the disease, it seemed to be as invariably absent.

*Dr Craig* remarked, that he had recently seen a case of dropsy, accompanied by vomiting, in which convulsions occurred, followed by motor paralysis of the left side; the use of bromide and iodide of potassium, with wine, sufficed to restore the patient in four days to a state of comparative health.

*Dr Hardie* said that he perfectly agreed with Dr Balfour in the necessity for a careful discriminating diagnosis of the cause of the paraplegia previous to the selection of the remedy to be employed. There did not to him seem any resemblance between Dr Stewart's cases and those of true ataxia. They appeared rather to resemble cases of ordinary diphtheritic muscular paralysis, especially in the suddenness of their occurrence; while true locomotor ataxy was always a disease of slow growth, and essentially progressive in its character.

*Dr Stewart* replied, that his cases presented throughout their whole course no symptoms of muscular paralysis, while the phenomena actually present were entirely those of ataxia—want of co-ordination.

XV.—*Dr Joseph Bell* then read his paper entitled OBSERVATIONS ON RAPID PULMONARY CONGESTION AS AN OCCASIONAL CAUSE OF DEATH AFTER SURGICAL INJURIES AND OPERATIONS, which appears in our present number, at p. 981.

*Dr Grainger Stewart* stated that he had frequently had occasion to dissect similar cases, but in none had he ever failed to discover some emboli in connexion with the lesions discovered.

*Dr Bell* replied, that a most careful search for emboli had been made in each of his own cases, but in each of them the search had proved unsuccessful, although conducted with the greatest care both by himself and an assisting friend; he was most decidedly of opinion that his own cases were not caused by embolism.

*Dr Bennett* remarked, that many cases of death from rare and peculiar causes were continually taking place. In one case, which had occurred under the care of the late Professor Miller, an elderly gentleman, shortly after entering a bath, expired suddenly, and on examination after death a firm clot was found to occupy all the branches of the pulmonary artery, without any other lesion whatever. In another body he, Dr B., had examined, the man died suddenly under the care of Dr Craigie. On dissection, his stomach was found filled with coagulated blood; while, not even on microscopic examination, could anything anormal be detected in the walls of that organ. He thought that the cases related by Dr Bell must be referred to some class of rare or peculiar cases, and that there was no special reason for connecting fatal pulmonary affections with surgical lesions.

# OBSTETRICAL SOCIETY OF EDINBURGH.

## SESSION XXIX.—MEETING VI.

*9th February* 1870.—Dr BELL, *President*, in the Chair.

### I. CHLORAL IN A CASE OF PUERPERAL MANIA. BY DR THOMSON, DALKEITH.

I HAVE a few jottings from which I shall endeavour to speak as to the use of chloral in a case of puerperal mania lately under my care. Mrs S——, aged 29, was delivered of her fifth child on the 4th December last. The labour was physically easy and natural. The first symptoms of mental disorder were noticed soon after conception—became more decided at the usual period of quickening—and continued much the same until a few days before labour—during and after which great wakefulness and restlessness took place. It has been noticed, that when this disease appears during pregnancy, a more severe form is to be apprehended after delivery. It was so in this case. From the time labour commenced, symptoms of acute mania appeared. She soon after became violent, and it was difficult sometimes to keep her in bed. To relieve this condition, and especially the insomnia, large and frequent doses had been given of morphia, sedative liquor of opium, and bromide of potass—but all in vain. It was in these circumstances, and at the suggestion of Sir James Simpson, that I tried chloral. For five nights and days I had been unable to procure sleep. While in this state of excitement, I gave her a dose containing only 30 grains, and had the extreme satisfaction of seeing her asleep in five minutes. She slept for eight hours; when she awoke she partook of food, and again slept quietly for four hours. She was now much less violent, but after some hours restlessness returned. A second dose of 30 grains was given, when sleep was again procured, but this time only for one hour. Two or three doses of 30 grains were subsequently given, but without the same decided effect as at first. I now gave 60-grain doses, and had the satisfaction of again procuring either sleep or quiet rest. On one occasion the patient became very cold after the larger dose, but there was no bad result. Sleep and food are the two great restorers in this disease. I was here indebted to chloral not only in procuring the former, but also in making it much easier to give the latter. Had sleep been procured earlier after the acute symptoms set in, the subsequent history of the case might have been different. From the time chloral was used, the disease became much less acute.

The patient has now been removed to an asylum. As there is no hereditary tendency to insanity one is entitled to hope for recovery, even though it may be much postponed. Baillie used to say, in a case of this kind, "that the question was not whether she was to get well, but *when* she was to get well." Two forms of puerperal mania are generally recognised—mania, more or less acute, and melancholia; the former occurring chiefly during and soon after delivery, and the latter when the system is suffering from over-lactation. In reference to prognosis in such cases, Gooch has well said, that "mania is more dangerous to life, melancholia to reason."

From my observations in this case, I look forward to chloral being of the greatest service in the acute stage of puerperal mania.

### II. CHLORAL IN A CASE OF PUERPERAL CONVULSIONS. BY DR MILNE.

On the 25th of January last, I was in attendance on Mrs ——, a healthy, but somewhat sensitive, woman, in labour with her fourth child. The case proceeded naturally and quickly, and the head was on the perineum, when a loud crash was heard in the house, caused by the falling of a ponderous lamp on the floor of the lobby. The suddenness and intensity of the sound, and also its unexpectedness, alarmed her exceedingly, and before she could be calmed down a violent convulsion followed, which lasted several minutes. The expulsive pains continued, and the child was speedily born. The placenta was also soon removed. Notwithstanding this, the fits continued with very short intervals and with considerable severity. I administered gr. lx. of the hydrate of chloral. The convulsions continued, though with less force and frequency, for about fifty minutes, when the patient fell into a heavy sleep. At first the breathing was sonorous, but gradually it became free, and the repose tranquil. The somnolent state lasted eight hours. There was no renewal of the fits. On awaking, the patient seemed rather confused, responding slowly to any question put, and it was with the utmost difficulty she could recollect the circumstance of the crash. She was free of headache or sickness. I may state that, on being slightly pinched while asleep, she winced unmistakably. She recovered well.

*Remarks.*—I report this case as it is the first time I have employed Liebreich's much-vaunted drug in puerperal convulsions—a remedy more hardly ridden at the present time than any other medicine in existence. The case was a purely psychical one, due to the shock of a sudden, loud, and unexpected sound, and therefore well fitted to test the powers of the medicine, these cases being deemed rather obstinate. On the other hand, there was no albuminuria, and the absence of this complication lessened in some degree the seriousness of the case, and increased its amenableness to remedies. The benefit I obtained from the chloral was considerable, although not sufficient, in my estimation, to send one into ecstasies, or impress one with the belief that a novel cure had been brought before us of unexampled power. Sleep was not secured for nearly an hour, and yet a pretty large dose was given. In a suitable case—in a non-plethoric person, say—I think that, with hydrochlorate of morphia, I could have induced repose sooner, and perhaps even banished the convulsions as well. There might, it is true, have been more of after evil effects (that is, with morphia) than in this case; but in other instances I know headache, muscular exhaustion, and vomiting have been observed. Then, with chloroform, a drug still unrivalled in many respects, equal power and benefit at least have been conferred in puerperal eclampsia. I may say, that I hold with Demarquay and others, that chloral is not an anæsthetic, or rather, perhaps, that it is not a practicable and safe one. The patient winces and suffers while under a very large dose of it, and I suspect, if you push it on to insensibility, you are beyond the line of safety, and, in fact, on the confines of death. It will be no rival of Simpson's potent elixir therefore, but may contend strongly with some of the narcotics, such as morphia. As a hypnotic, many have found it of unspeakable service, and Bardeleben of Berlin thinks that it will abolish opium altogether. Perhaps the eminent German is rather sanguine, and underrates the task of superseding a drug of such acknowledged power over acute suffering, and efficacy in many other ways. Time, however, will test and tell.

*Sir James Simpson* said that there was one circumstance which ought to be known in connexion with chloral—viz., that there are two kinds of the drug in the market, a cheap sort and a dearer sort, and only the best should be trusted to. He had taken it himself as a hypnotic when suffering from a feverish attack. In order to know how long it took to produce its effect, he took a book with him to bed, and he only got as far as the second page when he fell soundly asleep, and slept so profoundly that he did not wake when called at the usual time in the morning. He experienced no bad effects from it. There is one drawback to the use of the drug, however—that is, its tendency to produce nausea in some people. He had one patient who took it regularly for chorea, and she was in the habit of taking it with lemon-juice, and found that in this way no sickness was induced. A distinguished clergyman in town, who had long suffered from sleeplessness, now takes chloral, and sleeps for nine or ten hours. Sometimes the eyes become inflamed after using it, but that soon passes away. He had been told by Dr Balfour that he had used chloral with success in delirium tremens. Sir James thought that Dr Milne's was the first case in which the remedy had been tried in puerperal convulsions. He had himself tried it in labour, first on Christmas-day, in a long tedious case. One drachm was given, and then another; and though the patient slept an hour or two, the pains went on. In another case in which it was tried, the patient spoke away all the time, but afterwards declared that she had no recollection of the labour. Chloral has been used abroad by subcutaneous injection, and in a case in which this was done in the Infirmary it seemed to produce sloughing. It may likewise be administered by suppositories or pessaries. It is possible that by giving chloral smaller doses of chloroform might be needed. After the patient has fallen asleep care should be taken not to wake him up suddenly, as delirium is apt to ensue. Insufficient doses will sometimes cause delirium. Druggists prefer to give it made up in the syrup of lemon or orange. Chloral is not constipating. He thought that Dr Young's observations as to the influence of the drug in epilepsy were most important.

*Dr Cochrane* thought that, in a case like Dr Thomson's, it might have been advisable to keep the patient constantly under the influence of the drug.

*Dr Young* said he had used the drug with success in infantile convulsions, etc.

*Dr Ziegler* gave it to a child as a hypnotic, first an eight-grain dose, which had no effect; next night ten grains also failed, while ten drops of morphia sent her to sleep. He had given it also to a lady suffering from gastralgia, and in her case forty grains produced profound sleep, and left no bad effects further than tenderness of the eyes, which passed off.

*Dr Keiller* had a patient who suffered from intense neuralgia of face and head, for which she had taken Battly's solution, which did not agree. He tried chloral in twenty-grain doses, and after the second the patient fell into a sound sleep, which lasted six or eight hours. In the morning the eyelids were inflamed. Next night half a drachm was given, but that did not do so well. One great advantage of the drug was its power of inducing sleep rapidly, and this was of importance in many cases. He remembered a case of puerperal mania which passed off in three days, and in that patient chloroform was administered, and morphia given by the stomach-pump.

*Dr Mossop* said he had used chloral in some cases in the Children's Hospital. One child was brought in with a severe scald, and screamed incessantly. Three grains of the drug were given, and in five minutes the child fell asleep, and slept all night. The next dose had not such a good effect, and the third did no good at all. In a case of meningitis, chloral was given in ten-grain doses with some benefit at first, but it lost its effect. It was given dissolved in glycerine.

## III. THE ALLOA CASE OF DEATH UNDER CHLOROFORM.

*Sir James Simpson* gave some particulars of the case of death under chloroform which recently happened at Alloa. He said it was a case of ovarian disease in which Dr Brotherston was going to operate, and he had been requested to be present. He gave the chloroform himself, but the first incision woke the patient up. More chloroform was given, and the cyst was tapped; but just as Dr Brotherston introduced his hand for the purpose of turning out the tumour, the woman suddenly vomited, the pupils dilated, a faint came on, the pulse could not be felt, and the patient died. The question was, Whether she might not have died from a faint, even suppose chloroform had not been given? Many cases had occurred of patients dying suddenly under operations when no anæsthetic had been employed. He knew of four cases in Edinburgh where death had resulted from sudden fainting without any chloroform being administered at all; and when it was known that 8000 doses of chloroform were manufactured every day in this city by one firm alone, it was surprising how very few accidents occurred from its use.

*Dr Bell* asked if the sickness and faintness might not have been caused by the introduction of the hand into the abdominal cavity.

## DUNDEE MEDICAL SOCIETY.

THE last meeting of the session was held on Wednesday, 6th April, Dr GIBSON, President, in the Chair.

*Mr Campbell, L.D.S.*, explained and exhibited the effects of inhaling nitrous oxide gas. He said, "Dr Priestly first discovered nitrous oxide gas, but it is to Sir H. Davy we are indebted for our knowledge of its chemical properties, and for suggesting and so far proving its anæsthetic power. I daresay he had little idea how fully his prediction would be realized when he said, in his valuable and exhaustive work on this subject, 'As nitrous oxide in its extensive operation appears capable of destroying physical pain, it may probably be used with advantage during surgical operations in which no great effusion of blood takes place.' The fact that a dentist was the first to give this suggestion practical effect (which was done by Mr Wells in 1843)—the first to produce anæsthesia for surgical operations—and the fact that nitrous oxide gas has been much fostered by members of that specialty in surgery, is my excuse for venturing to bring the subject before this Society." After briefly stating the history of its introduction from America to Paris by Dr Colton, and from Paris to London by Dr Evans, dentist, who, in spite of much opposition in some high quarters, prevailed upon the London Dental Hospital to give it a fair trial, generously gifting £100 to provide nitrous oxide gas and apparatus—the favourable report, based upon upwards of 2000 cases, by the joint committee appointed by the Dental Hospital and the Odontological Society in December 1868—its introduction into other hospitals since then, and his firm belief that it will rapidly come into general favour among surgeons, as it is fast gaining the confidence of patients,—he said: "It would be but affectation in me to give any minute account of the chemical properties of this gas, as you are all familiar with it, experimentally at least; and should it have escaped your memory, you have only to refer to any class-book on chemistry to have your memories refreshed. My object to-night is mainly to show you, practically, a few of its advantages over chloroform. You will notice its rapidity of action in producing anæsthesia, being, in my experience, from 30 to 150 seconds. You will also notice how rapidly the patients will recover from its anæsthetic effects, varying from 20 to 150 seconds. It is more agreeable than chloroform; when pure, having no odour, and but little taste. I have never seen any coughing or struggling during its administration. It seldom produces nausea and sickness. I have only had one case of sickness, and that trifling, and I have given it in at least 400 cases. Generally, patients recover completely from all effects of the gas in from three to five minutes. It has disadvantages, such as the expense and cumbersomeness of the apparatus required for its use. This objection, however, is being rapidly got rid of. To-day, I received this bottle, which you see on the table, from Messrs Coxeter, London. It contains 100 gallons of gas, compressed into a liquid. The vessel weighs 9 lbs., and is only 12 inches long, by 3½ inches in diameter. In this form it is quite portable and easily administered, and must of necessity be absolutely pure. It has been used in London in this liquid form with great success by Messrs Fox, Clover, Coleman, and Braine. It is but due to the first named of these gentlemen (Mr Charles James Fox) to state, that he has done more than any other man in the profession in this country to bring nitrous oxide gas into general favour. In this liquid form it will not deteriorate with age, as at present, when kept in a gasometer. For short operations, such as tooth extractions, etc., I believe it is bound to come into general use, and more than probably also in general surgery. It is considered the safest anæsthetic for short operations by, besides the names I have already mentioned, Messrs Sansom and Kidd, both authors of standard books on Chloroform. It has already been given by men of eminence in prolonged operations. Mr Coleman has kept a patient an hour unconscious under its influence. Others have done so for twenty minutes, and so on. To prolong anæsthesia, the patient is put completely under with the gas *pure*, then mixing it with atmospheric air. When signs of consciousness begin to appear, the gas is turned on full, as when first given. I may mention, that the excitement which accompanied the administration of nitrous oxide gas, when given years ago as laughing gas, was the result of atmospheric air getting mixed with it while being inhaled. With Clover's finely-fitting face-piece this can easily be avoided. Excitement is now a rare accompaniment.

"As to the physiological action of the gas, doctors differ. Those concerned in this matter are daily looking with much interest for the remaining portion of the report already referred to. I am led to believe that it will be altogether in favour of the gas. Its physiological action on the blood will form a leading feature in the report.

"In the Dental Hospital of London alone, I believe it has been given, up to this date, in about 8000 cases. Mr Charles James Fox, in his private practice, has given it in 1500 recorded cases, and, to use his own words, 'without a hitch.' It is given largely on the continent of Europe, and extensively in America. By the end of 1868, Dr Colton had given it—he does nothing else—in 20,000 cases. In all, speaking advisedly, I believe 500,000 patients have been exempted from suffering by the application of this valuable agent; and, so far as I am aware, with but one fatal result, and that in a patient who was suffering from extensive disease of both lungs. I have myself given it in over 400 cases, and it is truly pleasant to see the thankfulness expressed in the countenances of patients, on awaking from their dreamy sleep, to find their tormenting enemy gone.

"The contraindications are so briefly stated in the joint report referred to, that I cannot do better than read them: 'Your Committee have to state, that so far as their present experience goes, they have met with nothing to absolutely contraindicate its use—having administered it to both sexes, at all ages, from one to sixty-six years, to patients of all temperaments, to women in different stages of pregnancy and suckling, to persons acknowledged to be the subjects of asthma, epilepsy, etc., and to persons labouring under many other constitutional derangements, without any ill effects following; but they would nevertheless recommend great caution in its administration to persons suffering, or presumed to be suffering, from organic diseases of the brain, heart, arteries, lungs, etc.; and to persons of very plethoric habit and short neck, such as would be considered especially likely to be injured by venous congestion about the head, even where no organic disease was supposed to exist.' They throw out this precautionary advice, not because they have as yet seen any untoward symptoms arise in such cases (although they have exhibited the gas in several such); but because they are of opinion that it would be in such cases, if in any, that untoward symptoms would be likely to arise from its exhibition."

Two patients then inhaled the gas, and had each a tooth extracted. The first, aged 26; complete anæsthesia, induced in 45 seconds, recovery in 105 seconds: the next, aged 25; complete anæsthesia, in 85 seconds, recovery in 160 seconds. Several members, after questioning the patients, expressed themselves as being highly satisfied with what they had seen of nitrous oxide gas as an anæsthetic.

*Dr Arrott* then proposed a vote of thanks to Mr Campbell, which was seconded by Dr Maclagan.

*Dr Begg* afterwards read a most instructive paper on "Idiopathic Gangrene," after which *Dr Gibson* proposed, and *Dr Arrott* seconded, a vote of thanks to Dr Begg.

# Part Fourth.

## PERISCOPE.

### SURGERY.

THE EXTERNAL USE OF CARBOLIC ACID has been recently fully discussed by the Medical Society of Berlin, in connexion with a statement by Professor Bardeleben of his experience of it.

Professor Bardeleben introduced the subject by a general statement of the case, in regard, first, to the fact that the injurious influences of suppuration do not depend solely upon causes solely corporeal in their origin, but also upon external influences. He then pointed out the various opinions in regard to these external influences, tracing the doctrine of purely chemical atmospheric causes from Hunter downwards, and referring to the means employed to counteract their injurious chemical agencies. He next detailed the views of Pasteur and others, in regard to the septic influence of microscopic fungi, and related fully Professor Lister's views in regard to this theory, and the means he has taken to prevent the injurious influence of their germs, the antiseptic veil, the antiseptic putty, etc. He then proceeded to give his own experience, with the threefold view, *1st*, of detailing its success in the numerous cases under his care in the Berlin Charité Hospital; *2d*, of pointing out that, besides the undeniable advantages of this method, it has several disadvantages; and, *3d*, of showing that this method is capable of greater development by the employment of other and better means than carbolic acid. In regard to the first head, he stated that the success of this treatment had been fully confirmed by the results attained in 241 cases then in hospital, 50 of which were serious cases, and three of them compound fractures, which, but for Lister's method, must have been amputated. In abscesses he had not always found the rapid cessation of suppuration described by Lister, but in very many it had occurred.

Under the second head, he stated that carbolic acid and carbolate of soda, either mixed with oil or dissolved in water, exerted an irritating and caustic action on the tissues, sometimes so great as to necessitate their disuse. Lymphangeitis and erysipelas occurred just as frequently under the use of these agents as without them, but did not seem to be due to their irritant action. On the other hand, they certainly dissolved chemically the waterproof tissues employed to prevent the soiling of the bed, and thus gave rise to great annoyance. This irritant action also acted injuriously on the granulations, producing ultimately a condition in which hæmorrhage, both superficial and deep, took place into them, involving their destruction and great delay in the cicatrization.

Carbolic acid, however, exerted a more important influence on the body generally; it was absorbed and acted poisonously in about one case out of ten. This poisonous action was revealed often so early as the second day by a peculiar action on the urine, which, pale when first secreted, became gradually darker and darker on standing. Albumen has been said to be present in such cases; but this was not found to occur in any of his patients, though special attention was directed to this point. On the other hand, the patients were found to lose appetite and strength, and where great suppuration existed, it became a serious question whether the treatment should not be given up for the sake of the patients' safety.

Professor Bardeleben said that it would indeed be a great advantage if we could procure a remedy which should possess all the antiseptic virtues of carbolic acid without its disadvantages, one which should not irritate the granulations giving rise to proud flesh and hæmorrhage, nor act injuriously on the general health; and he then proceeded to recommend the sulpho-carbolate of zinc, first employed by Wood, of King's College Hospital, as a remedy which seemed to possess all these good qualities. He himself had employed it in fifty cases, thirty of which were cases of great and copious suppuration. The suppuration was checked, the decomposition of the pus prevented, and no injurious effect was produced on the granulations, nor on the formation of the cicatrix. He had employed this salt recently in two amputations, in one case when, after the gouging of a thick-walled abscess of the tibia, protracted suppuration ensued, and in a host of minor injuries; he believed himself justified, therefore, in recommending further trial of this remedy. Wood had found it extremely useful in the suppuration of mucous membranes; he, Professor Bardeleben, had only treated two cases of blennorrhœa of the urethra with this remedy; in both of these, good results were rapidly produced, without pain or any other ill effect.

*Dr Zülzer* remarked that he had personally visited Professor Lister's clinic at Glasgow, a short time previously, and satisfied himself of the favourable results attained by this method. He had been particularly astonished by being told that the hospital, from being a perfect hotbed of pyæmia, was now almost entirely free of it.

Various other speakers made remarks upon the subject, *Dr Liebreich* directing attention to the benzole compounds as the most important source of new disinfectants.

*Professor V. Langenbeck* then stated, that although at first he had the greatest distrust of Lister's method, yet two years' experience of it had now so convinced him of its utility, that hardly any operation was now performod in his clinic without the use of carbolic acid. Too little attention had hitherto been paid to the antiphlogistic properties of this remedy. Its depressing properties, as already pointed out by Herr Bardeleben, were sometimes excessive. In one case of large abscess, when the acid solution was employed too strong (1 : 4, while 1 : 6 or 1 : 8 were on the average the correct proportions), great feebleness of the pulse, and dark discoloration of the urine came on, both of which disappeared on employing a weaker solution. Carbolic acid had also an indubitable local depressing action, the wound healing very slowly, and sometimes the granulations ceasing to grow, the wound looking quite flabby, so that this treatment had to be given up. The permanent water-bath, which fifteen years ago he had recommended, and had since then continued to employ in treating wounds with the most successful results, he had given up entirely during the last six or eight months, believing that similar results were attainable by the carbolic acid treatment in a manner more convenient both for patient and surgeon. Of late he had numerous injuries of the fingers, in which great swelling and distressing phlegmonous inflammation are so apt to occur, but all this was prevented by carbolic acid. He had obtained very striking results in a case of resection of the wrist, in which the soft parts were much swollen, and the portions of bone to be removed of considerable size. The complete suppression of inflammatory reaction in this case was undoubtedly due to the antiphlogistic action of the carbolic acid, whether that depended on the exclusion of germs, or on its local depressing action on the bloodvessels. He could confirm all that Herr Bardeleben had said in regard to the admirable action of carbolic acid in compound fractures: quite recently, he had two compound fractures of the leg, which, according to still prevailing doctrines, ought to have been amputated, but which both ran a favourable course under the carbolic acid treatment.—*Wiener Medizinische Presse*, 20th March 1870.

*Note by Mr Lister.*—The results of the antiseptic system of treatment in Berlin, as above recorded, must be regarded as, on the whole, very encouraging; while the failures and drawbacks alluded to by Professor Bardeleben admit of ready explanation in the imperfection of his means and methods. Thus, in treating abscesses, he has retained the piece of oiled lint beneath the putty,—a method which I certainly never advised, but, on the contrary, strongly deprecate. For, when the antiseptic oil has been washed out by the discharge, the lint opposes an obstacle to the action of the acid in the putty, and affords a nidus for the development of putrefaction. Again, the local irritation which he describes is a necessary result of the omission of the use of a "protective" to guard the wound from the direct action of the acid. Lastly, as regards the poisonous effects on the system generally, it is long since I witnessed any such phenomena. When I used the putty, the peculiar dark urine was often observed, but I have not seen it since lac plaster was substituted for the paste.

## MEDICINE.

In Itch, styrax has been recently strongly recommended, as was formerly stated in this Journal; more recently the balsam of Peru has been said to be equally successful, and from its liquidity more readily applied. It is more expensive than styrax, but, from less being required, it is actually a cheaper remedy. No hospital treatment for such cases is now required; the patient is stripped and well and carefully rubbed over with Peru balsam from crown to heel, avoiding the production of abrasions. It soaks into the galleries and eggs, and kills everything on the body within an hour. A second rubbing, ten days after, destroys any stray animalculæ or products of eggs that may by chance have been in the clothes at the first rubbing, and thus does away with any need for baking or otherwise disinfecting the clothing.

On Change of Type.—The reader who has carefully studied the medical literature of the last quarter of the last century, must have often noticed how nearly the pioneers of that day agreed in theory and practice with the advanced men of our own times. A change of type—changing from one indicating reduction and depletion to one demanding support—was recognised by many observant practitioners.

Dr R. Hamilton (Memoirs of the Medical Society of London, vol. ii., 1789) thus writes:—"I am apt to think our constitution considerably changed within this last century in Great Britain. Luxury and its enervating influences render the diseases of the climate less inflammatory than they formerly were. Diseases that in their nature were always, and even now are, allowed to be phlogistic, have appeared within the space of the last thirty years, accompanied with a considerable degree of putridity, as the measles."

After alluding to our change of diet and of habit, he concludes: "All these concur to strengthen an opinion I have for some time entertained, that our diseases partake much more of debility than of genuine inflammation, and that the system cannot bear the same evacuations as were formerly in use, even in diseases universally allowed to be of the phlogistic type."

Dr Sims, writing of the Scarlatina Anginosa (Memoirs of London Medical Society, vol. i., 1787), says:—"Of bleeding, much need not be said. That many cases would have borne it I have no doubt, from the whole complexion of the fever attending on them; and therefore I know it was many times practised without doing mischief; but it does not follow that it was requisite or did good. In my own practice, I began early almost totally to neglect it, from an observation that the cases where the pulse was the strongest, and heat and fever highest, were not those attended with the most danger, and never afterwards had recourse to it, except in violent peripneumonic cases, which indeed was very rarely blended with the disease."—"I am also certain that I have seen many cases become fatal by the abuse of bleeding, so that I think it much safer to prohibit it than to be at all free in prescribing it."

Mr Joshua Walker of Leeds (Memoirs of the Medical Society of London, 1789), in a paper on Atrophia Lactunteum, notices the same tendency to debility, and attributes it to the increasing tea-drinking propensities of suckling women.

Dr Martin Wall, Lord Lichfield's Professor (Clinical) at Oxford, in a letter on an epidemic at Oxford in 1785, thus writes:—"Considerable evacuations, either by the lancet or by purgatives, were almost always prejudicial, sometimes fatal. Some slight symptoms of inflammatory diatheses which accompanied the first accession of this fever induced practitioners of the best judgment to use the lancet, and I have seen the blood much inflamed; but I do not recollect in any instance any advantage resulted from this practice, and in many cases a fatal debility almost always supervened, which no cordials or restoratives could overcome."

Dr Withering, in a paper on Scarlatina (Medical Tracts, vol. viii., 1769), writes thus of bleeding:—"Such was the state of the pulse with us during the summer months that I have never seen a case in which blood was taken away, nor would it be easy to perceive with what view the boldest or the most ignorant practitioner would attempt it."

Dr Samuel Parr, in a paper in the same volume, says:—If we were to examine into the history of medical cases from the beginning of the world to this time, we should find perhaps that more injury than advantage had been sustained by bleeding; for in those cases where evident marks present themselves to indicate its use, other circumstances of great consequence to the constitution will prohibit it. I believe it often happens, that to cure the disease we destroy the patient."

Authorities might be multiplied; but sufficient has been advanced to prove, that in theory and practice worthy men of old were in accord with modern views.

How are we to account for the recurrent development of Broussaism, heroic doses of calomel and of antimony, the starvation diet, and the exhaustive bleedings, which intervened between their day and ours? How came it to pass, that during that period an entirely new school of thought sprang up, flourished, and decayed?

We must either abandon the notion of change of type, or allow that between 1770 and 1870 there have been two changes of type, one from an asthenic to a sthenic diathesis, and one back again from a sthenic to an asthenic.—*Compiled by Christopher Johnson, F.R.C.S., Lancaster.*

# Part Fifth.

## MEDICAL NEWS.

Medical Reform in various phases is agitating the profession throughout the world. The Canadians are in advance of us, and have already secured a Central Board and Special Examiners; all that they now want is the exclusion of sects—homœopathists, eclectics, *et hoc genus omne*—from legal recognition, Ontario being the only country in the world where medical sects are legally recognised,—better, perhaps, than persecuting them. Our brethren in America, on the other hand, are not only agitating for medical, but also for social reform; and it is much to their credit that they have seen their way to agitate for the supremacy of mind over colour. Having already abrogated all invidious distinctions of sex in this matter, it is only right that the claims of colour to a similar exemption should be fully recognised. And as this is a matter in which we, with our large Eastern and Western possessions, are deeply interested, we have much pleasure in reproducing the Memorial of the National Medical Society of the District of Columbia.

"*To the Senate and House of Representatives of the United States:*

"Whereas it has been stated in a published circular that the persons endeavouring to form a medical society on the basis of 'equality before the law' have maliciously and falsely attacked the Medical Society of the District of Columbia, we deem it but just to the public, as well as ourselves, to make the following statement of facts:—

"Within the past few years some coloured physicians, regular graduates of medical colleges, and of untarnished character and reputation, having held positions as surgeons in the Union army during the rebellion, have settled in this city, and secured to themselves a large professional practice.

"There being only one medical society in the district, where all licenses to practise must be obtained, and all advantages flowing from medical and professional discussions were to be enjoyed, it became the duty of these coloured physicians to obtain license and membership, in order to keep up their medical education, and derive all the advantages from weekly professional discussions.

"The Medical Society of the District of Columbia has, on two different occasions, refused to elect these coloured physicians to membership, acknowledging that the colour of the candidates was the reason for so doing; and some of its members have refused to consult with them because they were not members of the Society.

"This was in June 1869. Hoping that discussion of the subject would aid in securing justice, we were content to await the result.

"*January* 3, 1870, by a vote of 26 to 10, the Society refused to consider a resolution offered by Dr Reyburn, which read as follows, viz.:—

"*Resolved,* That no physician (who is otherwise eligible) should be excluded from membership in this Society on account of his race or colour.'

"Some of the present officers of the Society have refused to consult with the coloured physicians, but instead thereof have taken charge of patients who were under their care, without giving them the customary notice of their dismissal, in direct violation of the ethics of the profession.

"These coloured physicians have applied to the Society for membership, but were rejected by a large majority, although the Board of Examiners reported favourably on them. At the last election of officers in the Society, *held January 3, 1870, the Chairman of this Board* was removed, and a gentleman, late of the Confederate army, well known for his opposition to the admission of coloured physicians, was elected in his place, thus ensuring their future defeat. Other gentlemen who served during the war in the Confederate army are now prominent in the control of its affairs.

"At the same meeting, a white candidate, a gentleman of high professional standing, and occupying an important official position, was objected to, solely on the ground that he was believed to be in favour of the admission of coloured members.

"Again, the circular published by the committee of the Society states that their weekly meetings are 'social reunions.' These meetings are conducted under strictly parliamentary rules, from the opening to the adjournment, and only professional questions, essays, and papers are brought forward for discussion, *and gentlemen are even required to obtain permission of the President to retire from the meeting.* If these meetings, held in compliance with the charter of the Society, are only social reunions, then the meetings of all bodies not strictly parliamentary are social reunions.

"Other coloured men will soon graduate from medical colleges in the United States and throughout the world, and their rights should be protected and guaranteed within this district.

"It is a fact worthy of note, that this is the only country and the only profession in which such a distinction is now made. Science knows no race, colour, or condition, and we protest against the Medical Society of the District of Columbia maintaining such a relic of barbarism.

"We, for the reasons stated, and in accordance with the spirit of the times, ask Congress to grant a charter to a new Society, which will give all rights, privileges, and immunities to all physicians, making only the presentation of a diploma from some college recognised by the American Medical Association, and good standing in the profession, the qualifications necessary for membership."

Signed by R. REYBURN, M.D., *President;* Drs J. G. STEPHENSON, ALEX. T. AUGUSTA, D. W. BLISS, SILAS L. LOOMIS, PURVIS, R. J. SOUTHWORTH, J. TABER JOHNSON, J. E. MASON, *Committee;* C. A. GRAY, M.D., *Sec.*

COTTAGE HOSPITALS have been a great success, and are most useful to the communities among which they are placed. As an indication of the utility of such institutions, we subjoin the medical portion of the first report of the Alloa Hospital.

MEDICAL REPORT—*From October* 1868 *to October* 1869.

| No. | Name. | Age. | Sex. | Residence. | Disease. | Date of Admission. | No. of Days in Hosp. | Result. |
|---|---|---|---|---|---|---|---|---|
| 1 | R. W. | 39 | Male | Alloa | Morbus coxarius | Oct. 2, 1868 | 113 | Relieved |
| 2 | A. S. | 9 | „ | „ | White swelling | Oct. 2, 1868 | 352 | Cured |
| 3 | W. M'F. | 62 | „ | „ | Ulcer of both legs | Oct. 27, 1868 | 43 | Cured |
| 4 | F. S. | 22 | „ | „ | Simple fracture of right leg | Nov. 9, 1868 | 75 | Cured |
| 5 | J. M. | 58 | „ | „ | Cancer of finger | Nov. 17, 1868 | 14 | Cured |
| 6 | J. F. | 30 | Female | „ | Phlegmasia dolens | Nov. 17, 1868 | 15 | Cured |
| 7 | D. F. | 22 | Male | „ | Dislocation of left wrist, with fracture of radius | Nov. 26, 1868 | 31 | Cured |
| 8 | E. W. | 12 | Female | Tulligarth | Burn of left thigh | Dec. 5, 1868 | 99 | Cured |
| 9 | J. R. | 13 | Male | Alloa | Ophthalmia of left eye | Dec. 23, 1868 | 32 | Cured |
| 10 | A. F. | 52 | Female | „ | Whitlow of left hand | Jan. 11, 1869 | 24 | Cured |
| 11 | A. A. | 19 | Male | Clackmannan | Disease of left knee joint | Jan. 20, 1869 | 165 | Cured |
| 12 | M. D. | 50 | Female | Alloa | Hæmorrhoids | Jan. 24, 1869 | 49 | Cured |
| 13 | E. Y. | 44 | „ | „ | Fracture of the left arm and forearm | Jan. 29, 1869 | 32 | Cured |
| 14 | R. C. | 40 | Male | „ | Fracture of both bones of right leg | Feb. 5, 1869 | 32 | Cured |
| 15 | J. M'Q. | 19 | Female | Gaberston Farm | Acute rheumatism | Feb. 19, 1869 | 30 | Cured |
| 16 | J. M'C. | 49 | Male | Sauchie | Fracture of both bones of left leg | Mar. 6, 1869 | 34 | Cured |
| 17 | A. C. | 53 | „ | Alloa | Compound fracture of right leg—amputation | Mar. 26, 1869 | 6 | Died |
| 18 | D. P. | 31 | „ | „ | Wound of the forehead, with fracture of the skull | Apr. 4, 1869 | 49 | Cured |
| 19 | A. S. | 14 | „ | Clackmannan | Morbus coxarius | Apr. 15, 1869 | 82 | Cured |
| 20 | J. D. | 50 | „ | Tillicoultry | Ophthalmia of both eyes | Apr. 24, 1869 | 46 | Cured |
| 21 | J. B. | 27 | „ | Clackmannan | Neuralgia | May 5, 1869 | 18 | Cured |
| 22 | J. M. | 40 | „ | Tillicoultry | Rheumatism | May 11, 1869 | 16 | Cured |
| 23 | D. B. | 29 | „ | Kincardine | Internal hæmorrhoids, with chronic dysentery | May 17, 1869 | 20 | Cured |
| 24 | M. M'D. | 22 | Female | Alloa | Polypus of the nostrils | June 12, 1869 | 3 | Cured |
| 25 | J. M'Q. | 44 | Male | Kincardine | Anal fistula | June 25, 1869 | 28 | Cured |
| 26 | A. Y. | 54 | Female | Alloa | Irritable ulcer of right leg | June 25, 1869 | 29 | Cured |
| 27 | R. F. | 40 | Male | „ | Acute abscess of right leg | July 5, 1869 | 23 | Cured |
| 28 | J. M'A. | 20 | Female | „ | Housemaid's knee | July 9, 1869 | 29 | Cured |
| 29 | T. M. | 15 | Male | Fishcross | Burn of right eyeball | Aug. 12, 1869 | 45 | Cured |
| 30 | M. F. | 58 | Female | Alloa | Varicose ulcers of both legs | Aug. 10, 1869 | 49 | Cured |
| 31 | G. H. | 13 | Male | Sauchie | Curvature of the spine | Jan. 8, 1869 | | In Hosp. |
| 32 | J. S. | 18 | Female | Clackmannan | Disease of the cervical vertebræ | June 9, 1869 | | In Hosp. |
| 33 | J. D. | 26 | Male | Alloa | Railway accident—amputation of right leg above knee, and left leg below knee | June 22, 1869 | | In Hosp. |
| 34 | W. D. | 24 | Male | Clackmannan | Disease of left knee joint | July 29, 1869 | | In Hosp. |
| | | | | | *Fever Hospital.* | | | |
| 1 | J. H. | 31 | Male | Montrose—Mrs Henny's, Trongate | Typhus fever | July 7, 1869 | 16 | Cured |
| 2 | F. F. | 20 | „ | Germany—sailor | Typhus fever | Aug. 30, 1869 | | Cured |

*Remarks on the Cases referred to on the preceding page.*

No. 1.—This patient took a chest affection and had to leave.

No. 2.—This patient had the knee-joint excised, and has made an admirable recovery. He might have been dismissed some months sooner, but was detained to guard against the risk of using too much freedom with the leg.

No. 5.—Amputation of the finger was performed.

No. 8.—This was a very severe burn, and could not have been treated in her own dwelling satisfactorily.

No. 10.—Amputation of the little finger was required.

No. 11.—The actual cautery was applied with great benefit.

No. 12.—Ligatures were applied and a cure effected.

No. 17.—Amputation of right leg. This accident occurred in the morning; his friends took him to Stirling, and he returned to the hospital late at night, in such a state that the operation could not be performed till next day. Mortification set in, and he died on the fifth day.

No. 18.—In this case eight small portions of the frontal bone were extracted at different times. It is doubtful if this would have been a case of recovery had the man been treated in his own house.

No. 23.—This case had been regarded for two years as incurable.

No. 24.—Extraction was performed.

No. 25.—Cured by opening the sinus freely.

No. 28.—The tumour was excised.

No. 30.—This patient suffered for two years previous to her admission to the Hospital, and treatment in her own house would not have been successful.

No. 31.—Nearly cured.

No. 32.—Actual cautery applied.

No. 33.—This patient has made an excellent recovery, and is quite convalescent. He lost his left arm some time ago from a railway accident. This was the most critical case treated during the year, and would, in all probability, have terminated fatally but for the proximity of the Hospital.

No. 34.—Actual cautery applied twice; greatly relieved; the cure progressing.

*Fever Hospital.*

No. 1.—Fever prevented from spreading by isolating the patient, and fumigating and disinfecting the house from which he was taken.

No. 2.—This patient was on board ship, and could not be properly treated out of Hospital.

WE regret very much to have to record the death, from typhus fever, of Mr Edwin Thompson, M.B., C.M., M.R.C.S., Resident Physician in the Fever Wards of the Edinburgh Royal Infirmary, at the early age of twenty-two. Mr Thompson entered on his duties at the commencement of the winter session, and discharged them with great zeal and ability till struck down by the fatal disease of which he died. If true .greatness consists, as it does, in the faithful discharge of duty, the world is sadly unaware of the many heroes who fall, like Mr Thompson, unnoticed and unknown in the forefront of that battle which science is ever waging with ignorance and disease. A brighter day has already dawned for him; but the hecatombs of young and ardent lives which have already been sacrificed in the Fever Wards of the Edinburgh Infirmary, can only be fully atoned for when the public shall have awakened to their duty in regard to so fatal and yet preventible a disease as typhus.

APPOINTMENT.—James William Miller, M.D. Ed., and L.R.C.S. Ed., to be Surgeon to the Dundee Prison, *vice* Duncan, resigned.

WE are glad to be able to state that the health of Sir James Y. Simpson, which for some weeks past has given his friends great anxiety, continues to improve.

THE DEGREE of LL.D. was conferred on Dr Charles Murchison by the University of Edinburgh on Wednesday the 20th ult. We congratulate Dr Murchison on the honour conferred on him; but we cannot help saying, that there seems something amiss in a system of rewards which, because of his eminence in his own peculiar branch, makes a man a doctor of a science of which he probably knows nothing, and something specially rotten in making a man a Doctor of Laws because of his eminence in Medicine. We recommend the University, at their next graduation, to confer the honorary degree of M.D. on all our Senators of Justice.

THE UNIVERSITY COURT—Principal Sir Alexander Grant, Bart., in the chair—have recognised the Lectures on Surgery of Dr John Chiene, as qualifying for graduation in the University; and have approved of the appointment, by Professor Turner, of Dr Morrison Watson as Demonstrator of Anatomy.

## PUBLICATIONS RECEIVED.

L. F. E. Bergeret,—The Preventive Obstacle. Translated by P. de Marmon, M.D. New York, 1870.

Dr H. René Blache,—Essai sur les Maladies du Cœur chez les Infants. Paris, 1869.

William H. Flower, F.R.C.S.,—Introductory Lecture to the Course of Comparative Anatomy. London, 1870.

Wm. Warren Greene, M.D.,—Reduction of Dislocations. Boston, 1870.

J. Wickham Legg, M.D.,—A Guide to the Examination of the Urine. London, 1870.

Henry Alleyne Nicholson, M.D.,—Manual of Zoology. Vol. i., Invertebrate Animals. London, 1870.

Professor George Rolleston,—Forms of Animal Life. Oxford, 1870.

Edward Seguin, M.D.,—New Facts and Remarks concerning Idiocy. New York, 1870.

## PERIODICALS RECEIVED.

American Practitioner,—February to April.

Australian Medical Gazette,—January.

Births, Deaths, and Marriages, Monthly Return of,—March; Supplement for 1869.

Berlin. Klin. Wochenschrift,—Nos. 14 to 16.

British and Foreign Medico-Chirurgical Review,—April.

British Medical Journal,—March 26 to April 23.

Bulletin Général de Thérapeutique,—April 15.

Food Journal,—April.

Gazette des Hôpitaux,—Nos. 35 to 46.

Gazette Hebdomadaire de Médecine, etc.,—Nos. 12 to 15.

Gazette Médicale de Paris,—Nos. 13 to 16.

Journal de Médecine Mentale,—February.

Journal of Mental Science,—April.

Madras Monthly Journal of Medical Science,—March.

Medical Press,—March 30 to April 20.

Medical Times and Gazette,—March 26 to April 23.

New York Medical Journal,—April.

Philadelphia Medical and Surgical Reporter,—Feb. 5 to March 12.

Practitioner,—April.

Revue de Thérapeutique Médico-Chirurgicale,—No. 7.

Virchow's Archiv,—February.

# Part First.

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## ORIGINAL COMMUNICATIONS.

### ARTICLE I.—*On the so-called Synclitic Motion of the Fœtal Head in the Mechanism of Parturition.* By J. MATTHEWS DUNCAN, M.D.

(*Read before the Obstetrical Society of Edinburgh, 27th April.*)

IN an elaborate, ingenious, and valuable work recently produced at Berlin, by Dr W. L. Kueneke, and entitled, " Die vier Factoren der Geburt. Grundzuege einer Physik der Geburt," several alleged phenomena in the mechanism of labour are for the first time described. Of these, the synclitic movement of the fœtal head in its course through the true pelvis appears to me to be one of the most important. It is alleged to be followed by a quite opposite kind of movement of the head as it begins to distend the perineum, called the enclitic movement. It is my object at present to investigate this so-called synclitic movement, that it may be confirmed and accepted, if found to be true; and that it may be rejected, if found to be false, and the older views confirmed, elucidated, and explained. All this is particularly necessary, because the character of the progress of the fœtal head in the true pelvis, whether synclitic or oblique, has never attracted the attention which it deserves. In the sequel, indeed, I hope to satisfy the student that Kueneke himself, while fully alive to the importance of the subject, has omitted to bring to bear upon its discussion one of the chief agencies influencing it, and some collateral results throwing light upon it.

In order to explain, first of all, what is meant by the synclitic movement of the fœtal head in its progress through the pelvic cavity, I go back to refer to some points in its progress through the first part of the same bony canal. It is well known[1] that Naegele described the fœtal head as entering the brim of the pelvis obliquely as regards the plane of the brim,—that is, with its vertical axis not in the direction of the axis of the brim; with the plane of a horizontal section of the head not in the plane of the brim, but with the edge of this imaginary horizontal plane which looks anteriorly advancing first, with a lateral obliquity, in a position involving left lateral flexion of the fœtal head. Adapting Kueneke's nomenclature to this part of the progress of the fœtal head, we might say that Naegele described the fœtal head as entering the pelvic brim, not synclitically, but obliquely or antisynclitically. All recent authors, so far as I know, even including Kueneke, who have carefully studied this point, have differed from Naegele, and describe the fœtal head as entering the brim synclitically, not obliquely, with its horizontal plane in the plane of the brim, the plane of its vertex synclitic with the plane of the brim. It may be held as settled, that in the first part of its course through the true pelvis, the fœtal head does advance synclitically.

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[1] In the whole of this discussion, I shall assume that the advancing fœtal head is in the first position, the occiput directed towards the mother's left. It is scarcely necessary to add, that the kind of obliquity here considered is what Kueneke aptly designates the Naegele obliquity. The Roederer obliquity and the Solayres obliquity are not the subjects of this paper.

Very few authors have paid special attention to the exact condition of the fœtal head in this respect in its course through the pelvis. Those who have described it are, with the exception of Kueneke, so far as I know, of opinion that its progress is oblique, at least in the latter part of its course. Kueneke asserts that it is synclitic. It is generally held that when the fœtal head begins to leave the direction of the axis of the brim—begins to advance forwards—it advances obliquely, not with a point in the mesial line leading, but with a point in a parietal bone leading. Kueneke's doctrine implies that a mesial point always leads, a point in the sagittal suture.

If we define as the presenting point[1] that point on the surface of the child's head through which the axis of the developed pelvic canal passes; then, according to Kueneke's view, the presenting point is always in the mesial line, always in the sagittal suture, and must be so. According to most authors, the presenting point is, at least after the head begins to advance forwards, situated on the anterior parietal bone. If, indeed, the head becomes rotated so completely as to bring the sagittal suture into a direct antero-posterior position, then the presenting point will be, as in the passage of the brim, in the mesial line, in the sagittal suture, but only then. Here is a great difference between Kueneke and other authors. Kueneke describes the course of the head through the true pelvis as being synclitic; authors generally describe it as being oblique in the latter half of this progress.

The difference between Kueneke and others may be farther elucidated by stating it as follows:—Kueneke's synclitic motion of the fœtal head keeps the point of intersection of the antero-posterior and transverse diameters of that plane of the true pelvis through which the fœtal head is passing always in the sagittal suture. Other authors hold that the intersection of these diameters is in the anterior parietal bone, in the latter part of this course through the pelvis.

"Let us," says Kueneke,[2] "regard this canal of the pelvic cavity, curved as it is towards the symphysis, as divided by any chosen number of transverse-sectional planes, which the head has to go through, one after another, then, as these planes diverge in the sacral direction, the head makes in every succeeding plane a turning movement, whereby it is kept parallel to each plane as it reaches it, so that at length arrived at the last plane, close to the plane of the outlet, it stands parallel to this also." This synclitic movement is said by Kueneke to have the symphysis for its fulcrum, the transverse diameter of the head representing a single-armed lever moving on the symphysis as a fulcrum through an arc of 30 degrees, a figure which is said to be the measure of the difference between the inclination of the brim or inlet, and that of the outlet as defined by Kueneke, or of a line running in sagittal direction through the top of the pubic arch and the point of the sacrum (not of the coccyx).

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[1] Duncan's Researches in Obstetrics, p. 369.

[2] Vier Factoren der Geburt, s. 36.

"To attribute," says Kueneke, p. 38, "to the symphysis the importance of a point of support for the revolving lever, is, in my judgment, perfectly well founded; but withal, the mechanism of the synclitic movement is still by no means sufficiently elucidated. For there is still reserved the question, how at last the symphysis comes to be in a position to offer a resistance to the driving power; a question which has as yet not been once proposed, far less answered. I shall attempt to make out a solution of this not very simple mechanism.

"We know that the driving power falls at a right angle on the plane of the inlet, that its axis bisects its sagittal diameter, and passing through the cavity of the pelvis permeates the coccyx, and is inclined to the horizon at an angle of 30 degrees. Now, the matter would be very simple, if it could be shown that the symphysis was so situated as to converge towards this unchangeable direction of the driving power. If, to show this, there be drawn at the symphysis a line parallel to this direction, then there is at length found to be no convergence, not even parallelism, but, on the contrary, a divergence to about 10 degrees, of the symphysis, which is inclined at an angle of 40 degrees. Here, then, can therefore be no talk of resistance of the symphysis, and consequently none of synclitic movement. Much rather would this one-sided treatment of the relations establish the persistence of the orthophoric movement all through the pelvic cavity, and justify those who adhere to the theory of obliquity of the cranium in the pelvic cavity.

"As the symphysis does not, without some further condition, offer the expected resistance, we must look about for something else which may be fitted to afford to the symphysis the postulated peculiarity. This I find in the posterior pelvic wall. If we draw at the posterior wall of the pelvis, from the promontory onwards, a parallel to the direction of the driving power, it is found to coincide with the anterior surface of the two upper bones of the sacrum, passes diagonally through the third vertebra, and then runs behind the sacrum. There is thus from above to this point a lessening degree of friction between the advancing head and the two pelvic walls implicated, which compresses the skull in the direction of its transverse diameters; but as yet no one-sided resistance, which might produce a revolving. Evidently rather the friction begins first at the third sacral vertebra, and increases from hence downwards more and more. This friction is, nevertheless, still not in a position to produce a distinct resistance, which in case of its being in excess would act as a fulcrum; but the inclined planes of the excavated vertebræ exert, as the cranium glides over them, a pressure against the symphysis. This result has already been expressed by Roederer in these words: 'Vis sane composita oritur, ex dolorum impetu, qui in directione axeos pelvis agit, et plani inclinati et mobilis (sc. posterioris vaginæ parietis), cujus directio ad anteriora vergit, renixu.' Here, now, there would be produced a resistance, even supposing the symphysis formed a convexity corresponding to the sacral excavation. So much the more must this resistance come into play when the direction of the symphysis, in spite of its divergence from the direction of the driving power, has a not insignificant convergence with the direction of the lower part of the sacrum; through which last, as has been already shown, the direction of progress is in a corresponding manner inclined forwards; and this resistance, which, at the same time, is not strong enough to arrest the advance, forms the fulcrum round which, under a constantly diminishing progression from above downwards, the implicated transverse section of the cranium revolves as a single-armed lever, and so keeps in parallelism with each successive transverse-sectional plane of the pelvis into which it is brought in consequence of its advance. This is the synclitic movement."

I have thus given Kueneke's theory in a nearly literal translation of his own words. To what I have quoted, Kueneke adds very little further. Only, I may add, that he expresses his belief that, through the whole of the pelvic cavity, the head advances synclitically.

Before proceeding to discuss the statements and theory of Kueneke, I shall give a brief account of what I (in common, I believe, with many obstetricians) hold to be the facts in regard to this point in the position of the head as it passes through the cavity of the pelvis, from the brim to the outlet.

The fœtal cranium enters the brim or inlet directly or synclitically, that is, without lateral obliquity or lateral flexion. The presenting point is in the sagittal suture. The transverse and antero-posterior diameters of the brim intersect in the sagittal suture. Regarding only the matter now in question, the fœtal head advances, in unchanged relations, in the direction of the axis of the brim till it is arrested by the sacrum and the floor of the pelvis. In this way it often advances unchanged even till the vertex is nearly pressing on the coccyx. Up to this point its progress is direct, unchanged, because it has met with no special resistance; the vertical axis of the head is still parallel to the axis of the brim. So long as the head was passing through the upper half of the pelvic cavity, the plane of the vertex was synclitic with the successive parallel planes of that part of the pelvis. But there was no new mechanism to produce this synclitism. The head simply advanced in unchanged position thus far. As I have already said, it generally advances still farther in the same direction, and till it meets the resistance of the lower half of the sacrum and floor of the pelvis, maintaining its original position towards the axis of the brim; the axis of the brim still, when continued downwards, traversing the sagittal suture. But as soon as it so advances beyond the middle of the pelvis, beyond the transverse-sectional plane which passes through the middle of the third sacral vertebra, it ceases to be synclitic with the succeeding planes, and it never regains completely its original synclitism such as it had in the upper half of the pelvis. The axis of the second half of the pelvic cavity is never again (unless, indeed, the head so completely rotates as to have its sagittal suture lying exactly in the antero-posterior pelvic diameter) found traversing the sagittal suture, but traverses a point on the surface of the anterior parietal bone. The antero-posterior and transverse diameters of the transverse-sectional planes of the lower half of the pelvis never intersect in the sagittal suture.[1]

In making these statements of what I believe to be the facts in nature, I do not assert that there is no tendency or progress towards synclitism in the second half of the pelvic cavity. There is certainly such tendency, and I believe there is also, as a result of the tendency, an approach to the synclitic state. But the synclitic condition is never reached.

I shall now proceed to give the evidence upon which my statements as to the absence of synclitism in the second part of the progress of the fœtal head through the pelvic cavity (and, indeed, in its still further progress also) are founded. The great peculiarity of Kueneke's description of this matter lies in his assertion and attempted demonstration that there is a movement of the fœtal head keeping it synclitic as it advances. I believe it is not synclitic in the second part of its course.

I shall arrange the evidence I have to adduce under five different heads.

*First.* I shall appeal to observation, and in this, of course, Kueneke will join me. I may, however, describe a common condition, and assert that its truth will be generally recognised, and that it is quite inconsistent with Kueneke's views.

When, during an ordinary labour,—say, for example, a first labour, in which there is plenty of time for careful observation,—a digital examination is made when the vertex has reached the plane of the outlet of the ligamentous pelvis or of the pelvic excavation, or nearly reached it; when, in addition, as often happens, the case is favourable for the purpose, from the cranium having not yet begun, or only slightly performed, its regular rotatory movement on its vertical axis, it is found that the sagittal suture lying transversely, or more or less in the right oblique pelvic diameter, is far behind or posterior to the transverse diameter of the plane which the vertex or upper half of the fœtal head is occupying. The antero-posterior and transverse diameters of the plane occupied by the head do not intersect in the sagittal suture. The sagittal suture is intersected, or nearly so, by the axis of the brim prolonged downwards; it can be felt to be just above the coccyx. The anterior parietal eminence can be felt in the upper part of the pelvic arch, and nearly in the same pelvic plane as the sagittal suture. The sagittal suture is far behind the transverse diameter of this part of the pelvis. The axis of this part of the pelvis traverses the anterior parietal bone, upon which, therefore, lies the presenting point. The head is not placed synclitic with the transverse-sectional plane of this lower part of the pelvic cavity. The anterior side of the head is in advance of the posterior.

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[1] For another statement of the same facts, see my "Researches in Obstetrics," p. 358.

I choose the position of the head above described for two reasons, which render it suitable for testing and for disproving Kueneke's views. For at this stage of progress we still have the coccyx and other bony landmarks to indicate whereabout the axis of the developed genital canal lies. At a more advanced stage, these landmarks are absent, and the ascertainment of the position of the axis is rendered more difficult, on account of the head in its progress developing, and at the same time occupying, the passage, and thus introducing difficulty. At this stage of progress, also, there is often very little rotation effected, and the occurrence of rotation renders it difficult to judge regarding synclitism. Continued synclitism is conceivable during and after rotation, but Kueneke does not say it then exists.

Before leaving this part of the subject, I must make Kueneke's own testimony available against his synclitic doctrine. Kueneke describes very clearly the position of the sagittal suture after the head has undergone its rotation. This description he ends as follows:—"If we trace the sagittal diameter of the outlet upon the head as it lies in this position, its course will be, from the top of the pubic arch, about two centimètres distant from the sagittal point of the small fontanelle, starting from the right limb of the occipital suture, coming gradually to convergence with the sagittal suture near the forehead, intersecting it at its ending in the great fontanelle, passing over this last to the front of the left frontal bone, and on to its tuber, and further somewhat laterally to the left orbit." Now, this description by Kueneke is the description of a head which has certainly rotated, but which has not become synclitic. Kueneke does not notice this circumstance, and, of course, does not account for it. Were the head synclitic before rotation, it should be synclitic during it. But while the head is not synclitic, it is exactly situated as my views would lead one to expect. It is advancing with the anterior or right parietal bone, leading; with the posterior or left parietal bone, on a higher transverse-sectional plane of the pelvis than that occupied by corresponding parts of the right. Before it rotated it was not synclitic, and Kueneke's descriptions are erroneous. After it has rotated, it still is not synclitic or in the analogous state of its new position, and this accords with Kueneke's description just given, but is subversive of Kueneke's views;[1] for the axis of the genital passage traverses the right parietal bone. The horizontal plane of the child's head is not synclitic, for the whole sagittal suture is to the left of the mesial line. The axis of the pelvis does not pass through the sagittal suture, but through the parietal bone on the right side of the sagittal suture, just as it did before rotation commenced. It is thus evident that Kueneke has himself described the head in a position which is inconsistent with his own synclitic doctrine.

*Second.* Kueneke describes an enclitic movement of the fœtal head succeeding to the synclitic movement. While I do not admit the accuracy of Kueneke's description of this enclitic movement, so far as it is novel, yet, there is no doubt that it is, as Kueneke says, truly antisynclitic. It is opposed to the whole synclitic theory. The enclitic movement is observed after the rotation of the head, and consists essentially in a flexion movement. The synclitic movement involves comparatively rapid descent of the parts of the head lying near the posterior wall of the genital canal. The enclitic movement implies retardation or lagging behind of the parts situated posteriorly. Kueneke will, of course, reply to our statements, and to a great extent truly, that synclitism and enclitism are different conditions not justly to be compared. To this we reply, that if enclitic movement takes place, synclitic movement is thereby rendered improbable; that the same circumstances which produce the enclitic or antisynclitic state after rotation of the head prevent the synclitic state before the rotation. According to our view, the so-called enclitic state is merely a continuation and increase of the antisynclitic condition which previously existed.

*Third.* I have already, near the commencement of this paper, given, in a translation of a passage from Kueneke's work, that author's own description of the mechanism by which the so-called synclitic movement is supposed to be produced. To that description I object on two grounds; namely, that it is scientifically incorrect, and that it omits the chief agency that Kueneke should have invoked to support his own doctrine.

Kueneke's description of the supposed mechanism of synclitism is scientifically incorrect; for it assumes that the reaction is greater than the action. Kueneke says the posterior wall of the pelvic passage pressing on the adjacent left side of the head fixes, or nearly fixes, the remote right side of the head upon the symphysis, so that this right extremity of the transverse cranial diameter becomes the nearly fixed end of a lever represented by this transverse diameter. Now, supposing no other force acting, these pressures on both ends of this transverse diameter of the head must be equal and opposite, and in such a case there will be no tendency of the one extremity of this diameter to be more retarded by friction than the other. But there is another force acting, namely, the propelling power, whose direction is such as inevitably to make the pressure on the posterior wall or on the left extremity of the transverse cranial diameter, and therefore the friction there, much greater than on the anterior pelvic wall.

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[1] Kueneke's enclitic movement is supposed not as yet to have commenced.

But Kueneke's statement of the conditions and results is farther incorrect. If the head, arrived in the lower half of the pelvic cavity, is pressed forwards against the pubes, it is also pressed downwards by the driving or propelling power, and the resultant will not be a pressure against the symphysis leading to fixation by friction of the part of the fœtal head adjacent to the symphysis, but a tendency of the whole head to advance in the direction of the axis of the pelvis, or nearly so. Though we make this statement in order to show the error of Kueneke, we wish it to be understood, that we do not adopt the mechanism implied in our exposition as the best way of reaching a true view of the advance of the head.

That there arises from the mechanism a direct tendency to the production of the synclitic condition, there can be no doubt. This is demonstrated in the following manner:—In the lower half of the pelvic cavity, the genital canal begins to be curved forwards in the antero-posterior vertical plane. In consequence of this, the posterior part of the passage or the concavity of the canal is always nearer the line of the propelling force than the anterior part or convexity of the canal. The posterior parts of a body traversing the canal under these conditions will be acted on with more force than the anterior. Though Kueneke's alleged mechanism does not exist, there will necessarily be a tendency to the synclitic movement of Kueneke.

But there are two elements of this mechanism which prevent the synclitic movement. One of these is the circumstance that the fœtus is not a simple plastic mass, subject, without disturbance, to the conditions stated in the preceding paragraph. This plastic mass is furnished with a column of bones passing through it, which forms at least a main vehicle of the propelling pressure. So long as the fœtal head lies transversely, the statement of the preceding paragraph will be applicable, if there be no modifying circumstances. But the fœtal head generally has a Solayres diagonal position in this situation, and then the column of bones conveys the chief part of the force to the half of the head nearer the anterior wall of the genital canal, and the synclitic tendency is so far obviated. The increase of pressure on the half of the fœtal head nearest the pubes, in consequence of the Solayres obliquity, will not be great; but there is another reason why the synclitic movement does not take place.

The posterior half of the genital canal is the half where there is by far the greatest resistance to the progress of the head. It would not be very misleading to assert that, in that part of the course of the fœtal head, where synclitic movement is said to take place, the resistance is almost entirely in the posterior or concave side of the genital canal. In the middle, anteriorly, no structure has to be developed. There is only a minimum of resistance from friction. On either side, departing from the middle of the anterior wall of the passage, the resistance increases till the posterior wall is reached. There the development of the passage is greatest, and this development is the source of the resistance.

"On the other hand, however," says Schroeder,[1] "the resistances which the head experiences at the posterior and anterior pelvic wall are very different. At the entrance of the head into the small pelvis, the anterior and posterior pelvic walls run nearly parallel; the pelvis here may be regarded as a uniform cylinder, into which the head is driven. It is evident that, through such a cylinder, the head must pass in the same position in which it enters it; and this is what the head does so long as the pelvis answers to this description. But as soon as the head has completely entered it, the dissimilarity of the anterior and posterior pelvic walls makes itself remarked. At the posterior pelvic wall, the resistance which the head experiences increases; at the anterior it diminishes." It is this increasing resistance of the posterior pelvic wall which prevents the synclitic movement of Kueneke, just as the same resistance produces or maintains the subsequent alleged flexion-movement, or enclitic condition, as Kueneke designates it.

*Fourth.* There are some of the changes in the shape of the child's head—the result of the forces to which it is, during labour, subjected—which throw light on the questions discussed in this paper. Those to which I shall specially refer are the overlapping of one parietal bone over the other, and the shears, which have recently been described by Stadtfeld, Welker, and Dohrn—especially by the last named author.

It might be expected that I should, with this view, refer to the caput succedaneum, but a little reflection will show that it can give no aid. The caput succedaneum[2] can only show what part of the fœtal head was, during labour, not subjected to uterine pressure; it cannot show what was the presenting point, or where, on the surface of the head, the antero-posterior and transverse diameters of the passage intersected. The caput succedaneum shows what part of the fœtal head presented to or affronted the lumen of the as yet undeveloped part of the genital passage. If the axis of the undeveloped part of the genital passage (as at the brim of the pelvis) nearly corresponds with the axis of the pelvis or fully-developed genital canal, then the caput succedaneum will be at the presenting point. But if, as is the case in the second half of the progress of the fœtal head through the pelvis, the axis of the as yet undeveloped part of the genital passage does not correspond with the axis of the fully developed pelvic canal, then the caput succedaneum will have no relation to the presenting point, or that part of the surface of the head in the axis of the pelvis; but will show where was the axis of the undeveloped part of the passage.

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[1] Schwangerschaft Geburt und Wochenbett, s. 45.

[2] Researches in Obstetrics, p. 364.

The equitation or overlapping of one parietal bone by another is a subject which has, as yet, been insufficiently studied. In particular, sufficient observations have not, so far as I am aware, been made as to the comparative frequency with which the right overlaps the left, and *vice versa*. But it might, I think, be expected that, during labour, the left or posterior should overlap the right or anterior, if Kueneke's synclitic movement took place, for then the left or posterior would be pushed most downwards in the pelvis; while a compressing force acting laterally on the head would cause equitation: the left would overlap the right. But I believe it is generally the reverse: the right or anterior rides over the left or posterior. The overlapping of the left or posterior by the right or anterior is, however, just what would be expected in labour under the conditions which I have described, and attempted to prove to be the ordinary or natural ones.[1]

*Fifth.* The last evidence of the directions of the pressures to which the fœtal head is subjected is the shearing[2] (*verschiebung*), described by Dohrn.[3] I shall give the account of it in Dohrn's own words, merely premising that we do not entirely assent to all that he says:—

"It is," says he, "a known fact, that the skull is usually placed obliquely in the pelvis in such a way, that the side of the head lying anteriorly is situated more deeply than that turned posteriorly. It is also known that the side of the head directed towards the back is usually flattened, that directed forwards more strongly curved out. Both are consequences of the more powerful pressure which the head undergoes from the posterior wall of the pelvis.

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[1] "For ordinary," says Schroeder, *op. cit.* s. 103, "the parietal bone lying posteriorly is shoved under the anterior; but numerous exceptions to this occur even in the quite normal course of labour; especially in the first position of the head, the right parietal bone is not rarely shoved under the left; but, truly, in a normal pelvis there most frequently takes place no evident overlapping. In the pelvis, contracted in the conjugate diameter, with or without a less degree of general contraction, the anterior parietal bone is at least as frequently shoved under the posterior; while, in the equably uniformly contracted pelvis, the posterior parietal bone appears to be almost invariably overlapped by the other."

[2] Thomson and Tait's Natural Philosophy, § 171.

[3] Monatsschrift f. Geburtsk., bd. xxiv., s. 418, 1864.

"It is also this more powerful pressure of the posterior wall of the pelvis which produces the lateral shear of the cranium.

"If you look from above on a child's head which has been born in a cranial presentation, shortly after birth you will almost always find that it is oblique. This depends partly on the position of the caput succedaneum being mostly on a side; partly, however, it depends on a lateral shear of the two halves of the skull. This last you can easily make out, if you remark the position of the tubera parietalia. You will then find that the one stands much farther forwards than the other.

"The pressure of the posterior pelvic wall brings about this shear in the following way:—If I push downwards against the promontorium a fœtal cranium whose occiput lies deep down, its side directed to the promontory comes to be shoved forwards towards the coronal suture.

"If, on the other hand, I place the sinciput deep down, and now shove the cranium downwards on the promontory, then its side directed backwards towards the promontory is shoved towards the occiput. If I let the vertex advance first, and place the great and little fontanelle at an equal height, then there comes to take place a shear corresponding to the deep position of the sinciput, and always to the greater extent the more the head is inclined towards the forehead, the more directly from above you look at it, the less it appears to be direct.[1]

"This is the general law which regulates the degree and direction of the lateral shear. It is self-evident that it will be influenced by the energy of the pains, the size of the child's head, and the structure of the cranial bones; it is sufficient for me to point these out.

"Let us inquire now how far the law is confirmed by experience. I have for this purpose measured very many children's heads, and have noted carefully the appearances in the last forty which have come before me in normal pelves. During the first hours after birth, I placed a small cyrtometer horizontally around the greatest periphery of the skull, noticed at which links of the chain the frontal suture, the tubera parietalia, the occipital protuberance stood, and marked out the form of the cyrtometer on a paper divided into squares. In this way I found among the forty cases a lateral shear thirty-eight times; and it always happened that, where the half of the head turned towards the promontory had undergone a shear, pushing it forwards, there was during labour deep position of the occiput; where the reverse was the case, deep position of the sinciput was noted.

"The extent of this lateral shear in the first position of the cranium was on an average 5·3 millimètres; in the second position, where the occiput is originally more frequently directed backwards, and in consequence the pressure of the posterior wall comes more strongly into play, on an average 6·6 millimètres. At this point I must remark, that the different position of the tubera parietalia does not depend simply and alone on a shear of a parietal bone *in toto*, but to a small extent must be referred also to a change of place of the tuber in the bone itself.

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[1] The translator is confessedly at a loss as to the meaning of this sentence.

"In connexion with the frequency and the degree of the lateral shear, it may appear striking that it has not earlier attracted notice. This may well arise from the circumstance, that observers have allowed themselves to be deceived by the caput succedaneum, and particularly that the head is seldom carefully examined soon after birth. This last is necessary, if you wish to make out the lateral shear, for it quickly disappears, often already some hours after birth, and much earlier than the flattening of one half of the head in the vertical direction, which is often observed for a long time. The pressure of the brain, the elasticity of the bones and soft parts, as well as the tension of the muscles, co-operate to produce this disappearance.

"The importance which I attach to my observation is not merely scientific, but also diagnostic. If we examine a child's head shortly after birth, with reference to the mentioned influences, it will be possible for us to arrive at a retrospective conclusion as to its position during labour. The flattened side of the head is almost always that which was turned towards the promontory. If it is pushed forwards, then deep position of the occiput occurred; if it is pushed backwards, then there was deep position of the forehead. In this way it will be possible to control the results of examinations made during labour, and, in cases in which the labour was not personally observed, to form retrospectively a judgment as to the position of the head."

Before concluding, we must make a few remarks on Dohrn's valuable observations; and we must first ask our readers not to hold us as agreeing with Dohrn in ascribing to the promontory the place in the production of shear which he gives it. For us, Dohrn here is in a manifest error. Instead of the word *promontory*, we would place the posterior wall of the lower pelvis. In a natural labour the head is subjected to no kind of powerful pressure from the promontory. It is first subjected to such influences when it has passed through the upper half of the true pelvis.

Dohrn's description involves two shears, one vertical, the other lateral, which may be regarded as components of one shear.

The vertical implies the flattening of the posterior parietal bone and the bulging of the anterior. It is the result of the resistance offered by the posterior wall of the pelvis, which, as we have before seen, prevents the synclitic movement. The adjacent parietal is flattened, and the anterior parietal, bearing the mass of the caput succedaneum, is more than naturally bulged outwards.

The lateral shear, to which alone Dohrn attaches the name (*ver-*

*schiebung*) is merely that component of the whole shear which depends upon the Solayres obliquity of the head, and the rotation effected while the shear is in course of production.

Both shears at once disprove the existence of the synclitic movement, and help to explain why it does not take place.

## ARTICLE II.—*Cases of Acute Leucocythemia in connexion with Pregnancy.* By ROBERT PATERSON, M.D., F.R.C.P.E., etc.

(*Read before the Edinburgh Medico-Chirurgical Society, 4th May* 1870.)

THE following cases of acute leucocythemia occurred to me within a short space of time of each other. I was then in the belief that this disease was essentially a chronic one, and always marked in its commencement by splenic or other glandular enlargements. I was unaware that this disease was liable to linger in the constitution in a masked or sub-acute form, and unmarked by any easily-recognised symptom; until the occurrence of some change-producing event in the constitution, such as child-birth, after which the rapidity of its fatal course was to be measured, not by days, but by hours. In the two cases I am about to relate, death occurred in one on the eleventh, and in the other on the fourteenth day after delivery. Doubtless the disease was lingering in the constitutions of these women long before their confinement; but in the one case which only I saw before that event, there was no symptom which would have led a practitioner to predict a bad result, either during labour or afterwards.

M. Vidal has attempted to show that the first indication of this disease is to be found in enlargement of the spleen. Thus, of 17 observations, where the duration of this disease has been calculated between the appearance of the splenic tumour and death,—

| | |
|---|---|
| In from 3 to 6 months, . . | 3 cases. |
| „ 6 months to 1 year, . . | 5 „ |
| „ 1 year to 18 months, . | 5 „ |
| „ 2 years, . . . . . | 2 „ |
| „ 2½ years, . . . . | 1 case. |
| „ 4 years, . . . . . . | 1 „ |

The mean would thus appear to be 13 to 14 months, the minimum 3 months, and the maximum 4 years.[1] Thus the disease, according to most recent authority,[2] is essentially a chronic one, and only recognisable by the occurrence of splenic enlargement. I hope to be able to show, as the result of my inquiries into this particular division of leucocythemia, occurring in connexion with pregnancy,—that we have the means of determining the existence of this disease, even when the splenic tumour is not recognisable, or does not exist. I hope to be able to point out certain symptoms which will lead the practitioner to suspect this disease; and I can also refer him to a simple and certain means of verifying his diagnosis, viz., by means of the microscope; so looking to certain symptoms during the latter months of pregnancy, and ascertaining the state of the blood in the field of the microscope, much may be done in such cases to improve the condition of the circulating fluid before labour; to avoid that hæmorrhage which always accompanies labour under such circumstances, and which hastens the unhappy termination; and afterwards stimulate and nourish the system to a favourable result. To be aware of such a state of the circulating fluid beforehand is to be forearmed: the condition of that fluid, if it cannot be actually improved, may be prevented from increased degeneration by certain agents. Hæmorrhage during and after labour may be, if not avoided, much modified; and the condition of the circulating fluid rapidly improved afterwards.

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[1] Dictionnaire Encyclopédique des Sciences Médicales. Art. "Leucocythémie." P. 322.

[2] M. Isambert. See above article.

But if we wait for splenic or other glandular enlargements, we wait too long; they are in this class of cases recognisable only when the case is hopeless, and when they are assuming such rapid development as to lead to a speedy and fatal termination.

The first case I am about to relate—that of Mrs S.—was the first that had occurred to me in practice, and I candidly admit I was quite unaware either of its nature or danger.

Mrs S., æt. 20, an exceedingly robust and healthy young woman, married, and three months afterwards became pregnant. I was asked to see her several times during her early pregnancy, in consequence of severe sickness and vomiting and other symptoms, all referable to that state. These symptoms, however, gradually passed away, she enjoyed good health, was plump and well-coloured, and able for the usual amount of exercise. I was again asked to see her, about one month before her confinement. She had then a very sallow look, with hollow eyes, and rather rapid pulse (90); but I was assured that she felt quite well,—that although sometimes slightly feverish at night, and languid in the morning, she generally felt well and in good spirits; the urine exhibited no trace of bile.

The labour came on at the time expected, and was natural, the uterine contractions were vigorous, and she was delivered of a healthy male child, ten hours after the commencement of labour. Very troublesome and persistent hæmorrhage followed the expulsion of the placenta, which, however, was readily controlled by repeated doses of secale cornutum. For three days she continued to improve, and milk was secreted, the lochial discharge was scanty, and her pulse was high, but her condition at the end of this period was satisfactory. About the sixth day a marked change to the worse took place,—the pulse became more rapid (120), there was considerable heat of skin, with increased tawnyness of it; there was decided enlargement of liver and spleen, and some slight increase in size could be noticed in the glands of the neck, along with difficulty in swallowing, but to no extent. It was soon after this that, in consultation with the late Sir James Simpson, to elicit the nature of the case, we determined to examine the condition of the blood. This was accomplished next day.

The blood thus examined presented an unusual number of leucocytes or white cells in the field of the microscope,—so much so, indeed, that not a fourth part of the globules in view were red corpuscles. The microscopic examination speedily cleared up the diagnosis. We had no doubt a case of leucocythemia before us, indicated by the state of the glands and blood, and we had as little doubt that we had an unpromising case—one that would eventually proceed to a fatal termination; but we had no idea that, within twenty-four hours from the discovery of the disease, death was to put an end to further experience or treatment. Within this space of time, the principal change which took place was that of rapid enlargement of the glands of the neck; the difficulty of swallowing, before noticed, increased much, and any attempt to swallow in the horizontal posture produced such feelings of suffocation that she jumped upright in bed gasping for breath; at last she could not lie down at all; sitting up on the edge of the bed afforded the greatest relief; and when I last saw her, she was thus seated, restless and desponding, with great enlargement of the lymphatic glands of the neck, and with total inability to swallow even a teaspooful of water. Death took place suddenly on the evening of the eleventh day, apparently from suffocation and while sitting up in bed and gasping for breath, but with all her mental faculties clear to the last. The infant lived, and has been reared, and is now a healthy and robust child. A post-mortem examination was decidedly refused.

The case above related made me sufficiently alive to the necessity of watching cases exhibiting similar phenomena to the above, before their confinement, when I was asked to see Case II.

Case II.—Mrs M'C., the wife of a policeman, was confined under the hands of a midwife, who requested my assistance, in consequence of considerable hæmhorrhage after delivery. As this case occurred within a few weeks of the one I have just described, and as her appearance much resembled the other, I made a somewhat careful inquiry into her history and symptoms, as well as a minute examination into the state of the patient at the time. She was a primipara. She was originally of delicate habit of body, but was strong and well when she married. She suffered much from the early symptoms of pregnancy, and especially from severe sickness and vomiting, and for long she could take little or no food. Along with these symptoms, she became pale and languid; had to remain in bed the most part of the day, and became quite unable for any active employment about the house. Towards the close of her pregnancy, she became very yellow or sallow coloured,—so much so, as to induce her neighbours to declare that she laboured under jaundice; but some skilled old woman in the neighbourhood declared this was not so, from an examination of the urine. This history brings me up to the point at which I saw her after her confinement. She had lost a considerable quantity of blood; was pale and very sallow, especially around the mouth, nose, and eyes; but the blood which she had lost did not appear paler than usual; considerable enlargement of the spleen and liver was recognised, at the time I was exciting the contractile action of the uterus. Next day at my visit, I made a more careful examination of those organs, and also of the blood microscopically. The edge of the spleen was distinctly felt more than half-way between the false ribs and iliac bone, while the liver occupied an extended space of more than two inches below the ribs, occupying fully the half of the epigastrium, besides pressing considerably upwards. There was slight enlargement of the lymphatic, thyroid, or other glands. The pulse was rapid—120; the breathing laboured; no abnormal sounds in respiration; there was a bruit along the arteries in connexion with the first sound of the heart. The child was alive, and apparently healthy. The blood in the field of the microscope was full of white cells, and after a short time the blood-discs arranged themselves into rows, while the white or rather transparent cells occupied the greater part of the field of the microscope. It is no doubt loose language to talk of the blood being full of white cells, or containing a large or considerable number, but I had really no means of calculating the relative proportions of white cells and coloured corpuscles, so that I only state generally the impression it gave me at the time. This patient got all kinds of nourishment, with iron preparations and stimulants, but she never had any secretion of milk; the lochial discharge was pale and scanty; the fever increased, the pulse becoming gradually more rapid, with greatly increased restlessness; she could not sleep, and morphia and nepenthe failed to soothe her. Gradually the fatal enlargement of the lymphatic glands of the throat and neck and upper part of chest occurred, increasing slowly, and as surely gradually cut my patient off by asphyxia fourteen days after confinement. Death in this case occurred very much in the same way as in the former one, only more slowly and exhaustively. I had repeated opportunities of examining the blood of this patient from the day after her delivery till the day of her death, and I could not recognise any increase in the number of white cells or diminution of those of the red. A post-mortem examination in this case was also denied.

Since the occurrence of the two cases above described, I have been watchful over all the cases of pregnancy I have seen, and have especially noted and examined the blood of those who presented much sallowness of skin, much languor and inability for exertion, and I have contrasted this with a similar examination of the blood of pregnant women in whom no such symptoms were to be noticed: women of healthy and ruddy complexion, and free from languor and bodily inactivity. The result has been remarkable, in so far as that, in almost every sallow case, with complaints of weakness and listlessness, and yet without any recognisable enlargement of spleen, liver, or other glands, the blood has been found to be charged with leucocytes, while not a single white cell was to be seen in those of the ruddy and otherwise healthy women. Pregnancy, says M. Vidal, in four cases out of ten, is the commencement of the disease. Out of a number of cases which I have watched carefully during the latter months of pregnancy, the sallowness of whose skin, and the languor of whose system was specially remarkable, I shall give the history of one whose symptoms and the condition of whose blood gave me much anxiety, both as to the result of her labour and her recovery from it. Both the labour and convalescence have terminated favourably,—a result I could hardly at one time have contemplated.

Mrs L. consulted me early as to the state of her health. She stated that she was about five months pregnant of her fourth child; that she had had much hæmorrhage with her last baby, from which she had never entirely recovered; that her present ailments were principally languor, lassitude, and inability for any exertion, and, with the exception of a slight cough, she had no other symptom of the least consequence. She presented a pale sallow complexion, with feeble and rapid circulation, poor appetite, some morning sickness, with tendency to faint; no disease was discoverable in the chest. I had just become cognisant of the effects of leucocythemia in pregnant women, and very shortly afterwards took an opportunity of examining the blood of this patient under the microscope, and also of ascertaining the condition of the spleen, liver, and other glandular organs. This examination led me to the discovery of a large area of white cell-blood, but without being able to recognise any enlargement of the liver or spleen; there was, however, a certain amount of turgescence of the glands of the neck—a symptom which materially increased during the latter stages of her pregnancy, but was unaccompanied with difficulty of swallowing. The thyroid and submaxillary were the glands most affected. The treatment recommended was iron tonics, with stimulants, and abundance of carriage exercise in the open air. I cannot say that much improvement resulted from this means, for she continued to increase in sallowness and general weakness. At last the anxious period of her confinement arrived. I had determined to resist the use of chloroform, and to give ergot as soon as the state of the parts admitted; so, when labour advanced to nearly the full dilatation of the os uteri, I gave repeated and full doses of this drug. The result was satisfactory; the pains soon assumed a persistent character, and the child was delivered, after a labour of six hours' duration, well and healthy. The uterus contracted firmly, the placenta was thrown off naturally, and no post-partum hæmorrhage occurred. Liberal diet, with wine, was allowed from the moment of her confinement, and a slow but steady recovery followed. During the period of nearly four months over which we have travelled, the state of the blood was frequently examined, and I could not say that on any of these occasions I could discover a smaller number of leucocytes than existed at my first trial. There seemed always to be about the same number of white cells and rows of blood-corpuscles. I also examined the blood lost per vaginam at her confinement, and found it to contain about the usual number. Since her confinement, however, there has been a decided diminution of white cells, her countenance has assumed a more ruddy and healthy appearance, and she has been able for some little exertion, without fatigue. Six weeks afterwards she was sent to the country, and I have heard that she is steadily progressing to complete recovery. I may state that the enlargement of the glands of the neck was somewhat persistent, not having diminished materially, although they gave little discomfort, when I last saw this patient.

It is impossible for me to say what element in the history of this most interesting case is the one most conducive to practical results or to future practical guidance. My own impression is that, by saving the blood after confinement, we gained, as it were, the first step to future good results. All, it will be observed, was most unsatisfactory up to the period of confinement; but after that event, which was accomplished without prolonged suffering, and with no subsequent hæmorrhage, a steady and favourable progress was initiated, and a naturally good constitution allowed a fair start towards recovery. How these leucocytes are thrown off I do not pretend to determine; but as Virchow is clearly of opinion that they are never under any circumstances converted or changed into ordinary blood-corpuscles, so we must presume that they are eliminated from the body by some other means, most probably by the lungs. Still I should suggest that other means used in this case ought not to be neglected, such as the persevering use of iron, stimulants, and other forms of nourishment, both administered before and after confinement. The effect of this disease is curious as regards the fœtus. The mother's blood was charged with leucocytes, and consequently there was a proportionate diminution of the blood-corpuscles in the maternal circulation, yet the children suffered in no way, and were robust and healthy when born. This was the result of my three cases, demonstrating as it does most clearly the important physiological fact, that there is no direct communication between the blood of the fœtus and that of the mother.

# ARTICLE III.—*Case of Pseudo-Hypertrophic Paralysis (of Duchenne) occurring in an Adult.* By D. DYCE BROWN, M.A., M.D., Medical Officer to the Aberdeen General Dispensary, Fellow of the Obstetrical Society of London.

(*Read before the Aberdeen Medico-Chirurgical Society, 7th April* 1870.)

THE following case is extremely interesting, as being a well-marked example of a very rare disease,—a disease which has only been known to exist since 1861, when the attention of the profession was directed to it by Dr Duchenne (de Boulogne). But the case is still further interesting, in fact unique, as showing the disease occurring in an adult, whereas it has hitherto been believed to be exclusively a disease of infancy and childhood. Dr Duchenne, to whose elaborate and able researches, published in the form of a monograph on the subject,[1] we are indebted for all our information on the disease, says (p. 114, *op. cit.*):—"During more than twenty years that my researches have specially been made on affections of the muscles, and that, I may say, on a very extensive scale, I have never observed in the adult anything analogous to the pseudo-hypertrophic paralysis of infancy. In short, from what has already been said, there follows the important fact, that pseudo-hypertrophic paralysis is a disease of childhood."

The case is as follows:—J. C. M., æt. 26, was admitted into the Aberdeen County Prison on 2d June 1869, under a sentence of nine months' imprisonment. He had been at first a gardener by occupation, but for the last four years he had been a book-hawker. In this latter occupation he had not had any excessive amount of walking, and had been quite healthy up till about a month before being sent to prison. About this time (end of April) he began to feel "stiffish" in walking, had pains "like rheumatic pains" in the outside of the thighs, and found himself often involuntarily rubbing them. About this time he had a gonorrhœa, and on admission to prison he had eczema of the inside of both thighs, which was soon cured. On admission, he complained of pains in the lower part of the back and thighs, which might, however, have been caused by the eczema. He continued to feel his walking difficult, and his legs were getting weak; but he did not observe any muscular enlargement till about the beginning of July, a month after being sent to prison. At this time he told me that he thought his thighs were much larger than natural. He did not sleep well, looked heavy and stupid, but ate his food well, and his bowels and other excretions were normal. I examined his thighs at this time, and found them to be unmistakably larger in girth than natural. To the feel, the muscles felt firm, almost hard. Three weeks later, the calves of the legs had followed in the same enlargement, and were very firm and hard to the feel, the unnatural size of the thighs and calves being at once visible on inspection. The oblique abdominal muscles were also visibly enlarged. About the beginning of September, he complained of his arms feeling weak, and that he was unable to do his task. I found the biceps of both arms larger than natural, and rather firm, especially on flexion. The forearm was not enlarged, but rather the reverse, and the hands were thin and skinny. He had difficulty in closing his hands, and the grasp of both hands was extremely weak. When holding anything heavy in his hand, he felt considerable difficulty in preventing its fall. He complained now of no pain in his arms, thighs, or legs, but of general weakness, inability to use his hands, with stiffness and difficulty in walking, which he did with a slow, waddling, side-to-side gait, and with the legs far apart. There was no pain or tenderness in any part of the spine, no anæsthesia, no increased difficulty in walking with the eyes shut, no headaches or other "head symptoms." The skin was of normal colour. I did not take the measurements of the limbs till 27th October 1869, when the size of each was as follows:—

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[1] De la Paralysie Musculaire Pseudo-Hypertrophique, ou Paralysie Myo-Sclérosique. Par le Dr Duchenne (de Boulogne). Paris, 1868.

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|---|---|---|
| Girth of arm at middle of biceps, when extended, | 10 | inches. |
| " " when flexed, . | 11½ | " |
| The same in both limbs. | | |
| Girth of thighs opposite the trochanter major, . | 23 | " |
| " at middle, . . . . . . | 22 | " |
| " at lower third, . . . . . | 19 | " |
| The same in both. | | |
| Girth of calf of right leg, . . . . . . | 15½ | " |
| " " left leg, . . . . . . | 15 | " |

*12th January* 1870.—State much the same, except that the enlargement of the oblique abdominal muscles is more marked. The muscles of the back do not seem to be affected. The glutei had probably been affected at this time also with hypertrophy, although I did not observe it till a month later. There is no albumen in his urine, which has a specific gravity of 1·020, and is acid. No difficulty whatever in micturition, and action of the bowels regular and natural. He complains now of weakness of sight, especially of right eye. As there was no convenience for ophthalmoscopic examination in prison, this was deferred till his release, which took place on 9th February 1870. He then came to my house, where I examined his state again. The measurements of the limbs were found to be the same as on 27th October 1869, except that the girth of the calves was the same in both limbs, viz., 15½ inches. The glutei muscles were now visibly larger than usual, but the muscles of the back seemed unaffected, and there was not the deep anterior curvature in the lumbar region which is usually observed, and which is produced by the involvement of the muscles of the back in the paralysis. The state of the eyes, as reported by Dr Alexander Ogston, the Ophthalmic Surgeon to the Hospital, who kindly examined them, was as follows:—

*Right eye.*—S. = $\frac{10}{70}$. Retinal vessels small; pigment external to optic disc. Otherwise healthy.

*Left eye.*—S. = $\frac{10}{78}$. Vessels of retina small. Papillary edge of choroid showing pigment maceration. Colour vision perfect.

The grasp of his hands was at this time, if anything, weaker than before, and the movements of the fingers in flexion and extension, especially the former, difficult. After sitting down on a chair, he had considerable difficulty in rising, and for a few minutes after felt very stiff in walking. His walk was markedly of the side-to-side waddling character. He could only walk very slowly,—running, he felt, was out of the question. He had much difficulty in pulling off and on his boots.

I should state that his weight on admission to prison was 157 lbs., and at his discharge 162 lbs., showing an increase of 5 lbs. He was going home to the country, as he felt unable for any work; and in case I should not again meet with him, I think it better to record the case at this stage.

*Remarks.*—This disease is so rare, recognised as a definite disease so lately, and so little generally known, that in the way of making a few remarks on the foregoing well-marked example of it, the profession may perhaps think that I am not uselessly occupying the pages of this Journal by giving a general sketch of the disease in question; and in doing so I shall simply give a resumé of Dr Duchenne's interesting memoir.

That there existed such a disease as pseudo-hypertrophic paralysis was not known till so recently as 1861, when that very able observer, Dr Duchenne, met with a case of it, and recorded it. Since then, he has in his memoir collated thirteen cases, partly from his own practice, and partly from those of other physicians. Other cases have from time to time been recorded in various journals. All the cases recorded by Dr Duchenne commenced in early infancy or in childhood, the most advanced age of *commencement* which he has met with being ten years.

The general phenomena of the complaint are as follows:—There is—

(1.) A stage of general feebleness and weakness of the lower limbs.

(2.) There is a peculiar gait of side-to-side waddling, with unnatural separation of the limbs in walking.

(3.) A deep anterior curvature in the lumbo-sacral region of the spine, when in the upright position.

(4.) A state approaching that of talipes equinus, with clawed nails.

(5.) The muscles of these limbs enlarge to a very marked degree; are apparently hypertrophied.

(6.) There comes a period when the symptoms remain stationary.

(7.) After this, the paralysis becomes general over the body, and increases progressively in degree, the patient ending by being unable to move himself off the couch, and generally dying by some intercurrent disease, which his debilitated condition renders him unable to resist.

Let us look more particularly at each of these symptoms.

Those attacked by the disease have generally been in perfect health, and have been fine, healthy-looking children. No pains or fever precede the morbid state, and in only two of the recorded cases did an attack of convulsions occur shortly before the symptoms of the malady appeared. The first symptom in order of time is the weakness of the lower limbs. If the child is under the age when walking would be looked for, the enfeeblement may be entirely overlooked until this period arrives, when it is found that it cannot stand, and does not, as usual, instinctively move about its limbs, but cries, when put down, to be taken into its nurse's arms. This state is apt to be thought little of by mothers when they find that the little limbs, instead of growing thin, are large and firm. If the weakness commences at a later period, the child gets easily fatigued in standing or walking, falls frequently, runs with difficulty, or not at all, and finally will not walk, but desires to be carried.

(2.) Next we have the separation of the lower limbs, and the lateral balancing, or side-to-side movement in walking.

The separation of the limbs is an instinctive means of keeping up the equilibrium, and strengthening the base of the body in motion. This may occur in other diseases when the lower limbs are feeble. The lateral balancing or side-to-side waddling movement in walking, is caused by the weakness of the muscles of the back and buttocks. This also is observed in other diseases, but it is never to the same extent, and instead of soon disappearing, in the disease we are now speaking of, it only becomes aggravated.

(3.) There is the spinal curve when standing upright and walking. This is one of the most constant symptoms of pseudo-hypertrophic paralysis, but is only present during walking, or in the erect position. During the first period of the disease, there is only a slight bending backwards of the trunk, which by degrees increases till a deep lumbo-sacral curvature is formed in the upright posture. This curve in the lumbo-sacral region of the vertebral column is caused by the paralyzed condition of the extensor muscles of the spine, which generally exists in this complaint. Whenever these extensor spinal muscles are paralyzed, then the curve is produced. What proves that this is the real cause of the curve is, that Dr Duchenne found that in all cases where it existed, if, in making the patient stand upright, he tried to prevent the spinal bend, the trunk fell forwards, and the patient could not rectify his position without the assistance of his hands upon the legs and thighs. It is then a natural result of the effort to keep upright; the patient bending backwards, and supporting the body by means of the abdominal muscles, which act as flexors of the spinal column. When a patient having this curve stands upright, a plumb-line touching the spines of the vertebræ at the most prominent point falls far behind the sacrum. Dr Duchenne calls the curve thus produced "paralytic lordosis of the lumbo-spinal muscles," to distinguish it from that curve produced by weakness of the abdominal muscles in other diseases; in which case, though a deep lumbo-sacral curve may be formed, a plumb-line applied as above falls over the centre of the sacrum.

(4.) Equinism, or talipes equinus. This, Dr Duchenne says, is one of the most constant phenomena in the disease in question. It does not appear in the early stages of the malady, but, beginning very slightly, it increases gradually till the heel with difficulty touches the ground when in the standing posture. Then afterwards it takes the form of talipes equino-varus. The plantar arch is extended, the first phalanges are placed in a position of unnatural extension with respect to the metatarsal bones, while the two last phalanges are unaltered. The toes thus take the appearance of claws. The cause of this equinism, Dr Duchenne has satisfied himself, arises from the excessive predominance of the extensors of the foot over the flexors in this disease. He found that flexion of the foot was performed with difficulty, and feebly, whereas the reverse is obtained in extension.

(5.) The apparent muscular hypertrophy. This is perhaps the most striking feature in the whole disease,—the existence of enlargement, firmness, and even hardness of the voluntary muscles, along with a state of extreme weakness. This apparent hypertrophy always follows in point of time the state of well-marked feebleness, which is observed as the first symptom. Any of the voluntary muscles may be thus attacked with apparent hypertrophy. It generally begins in the lower limbs, in the gastrocnemii, in the thighs, and the buttocks; then the lumbo-spinal muscles may follow; the oblique abdominal muscles also, and the biceps of both arms, as in my case; the deltoid, and even the muscles of the face, especially the temporals. But while we find some muscles hypertrophied, we may find others emaciated. Thus the muscles of the upper extremities may be smaller than natural, while those of the lower are hypertrophied. In my case, the biceps of each arm was enlarged, while the forearm was rather thinner, and the hands quite skinny. In other cases, the pectorals and the sterno-mastoids have been found atrophied. Thus, it appears that although nearly all the voluntary muscles may be paralyzed, it does not follow that they should all be hypertrophied; or, in other words, the degree of paralysis does not proceed in the direct ratio of the hypertrophy. The description of the anatomical state of the muscles causing this apparent hypertrophy, I shall reserve till we finish the consideration of the general symptoms.

(6.) Then comes a period when the progress of the disease comes to a standstill for a varying period, generally several years, during which time the hopes of the friends are apt to rise. But—

(7.) After this stationary period, the disease starts afresh. The weakness of the lower limbs increases till the patients are unable to stand upright, and are confined to their bed or couch; the upper limbs become likewise enfeebled, and often become meagre and atrophied, so that the patients are in a nearly helpless condition, during which they are cut off by some intercurrent disease, such as phthisis. There are no cerebral symptoms which are uniformly present. In some cases there has been slowness of speech, with blunting and dulness of the intellectual powers, even idiocy, while in others the mind has been perfectly intact.

The electro-muscular contractility is at first quite normal, and it is only in the advanced stage of the disease that this is diminished or lost. The temperature of the enfeebled and hypertrophied muscles is generally lower than normal, and the skin has sometimes a rosy or bluish tinge, but neither of these phenomena are constant. It is also to be observed that, in general, no pain is complained of, and there is neither hyperæsthesia nor anæsthesia. The functions of the bladder and rectum remain normal, and the general health is only affected at an advanced stage of the disease.

We come now to inquire what are the anatomical alterations which are found to exist in pseudo-hypertrophic paralysis. As to the state of the nervous centres, the results are purely negative. Only one autopsy has hitherto been obtained, and in it no lesion of the nervous centres was detected. Then, as to the condition of the muscles: this has been ascertained in the dead body, and in several other cases in the living body also. This latter has been accomplished by a peculiar instrument, used by Dr Duchenne for extracting a piece of muscular tissue from the living body, and called by him "emporte-pièce histologique."

In the first place, the apparent hypertrophy of the muscles has been found to be the result, not of increase in number or size of the muscular fibres, but of an hypertrophy of the connective tissue, which normally exists. This is what occurs in the second stage of the disease, namely, when the muscles begin to enlarge. Besides this hypertrophy of the ordinary connective tissue, there is also a formation of numerous fibres, with fine undulations. A little further on, in the course of the disease, disseminated among the connective and fibrous tissues, there are found fatty particles of different sizes, and sometimes drops of oil, in clusters.

Next, as to the state of the muscular fibres themselves, they are found (1.) discoloured, presenting a pale appearance, and resembling the subcutaneous cellulo-adipose tissue. (2.) The normal transverse striation becomes extremely fine, and in some parts hardly to be seen, till (3.) the transverse striation completely disappears. (4.) The transverse diameter of the muscular fibres becomes very much diminished; there is, however, no parallelism between the diminution in the transverse diameter and the fineness of the striæ, as it has been found that in fibres whose transverse diameter has been normal the striæ have become very fine, and have even entirely disappeared. (5.) There is a marked diminution in the number of muscular fibres in proportion to the quantity of hypertrophied cellulo-adipose tissue. Griesinger found that in one case the intermediate tissue occupied six or eight times larger space than the muscular fasciculi. If the muscular fibres had not really degenerated, the size of the muscles involved ought to be larger than it is, judging from the enormous hypertrophy of the intermediate connective and fibroid tissues.

But there is good reason to believe that the muscular fibres do really degenerate and disappear, and that probably these undulated fibres, which exist among the connective or areolar tissue, are in reality what remains of empty sarcolemmata. The fatty globules, however, are not part of the muscular fibres in a state of fatty degeneration, but are entirely outside of them. The anatomical state of the muscles shows completely that it is an entirely different disease from that known as progressive fatty atrophy of the muscles. The symptoms of the disease are so marked, and so different from those of any other, that the diagnosis is hardly to be mistaken.

What, then, is the ultimate cause of this muscular disease?

It has been already mentioned that no lesion of the brain has been found; and although in some cases there was obtuseness of the intellectual faculties, even going the length of idiocy, yet in others there was no symptom of this sort, the mind being quite intact.

Besides, as Dr Duchenne remarks, a cerebral lesion would not be sufficient to account for the morbid condition of the muscles. He also observes that, reasoning by analogy, we should not expect to find the seat of the disease in the spinal cord; and at the autopsy above referred to, the spinal cord was found intact. Dr Duchenne is inclined to place the seat of the disease in the vaso-motor nerves, and states that he cannot conceive of the production of such an increase of connective tissue, unless the vaso-motor nerves are involved and paralyzed. The facts that in some cases the skin has been of a rosy or bluish colour, and that the temperature of the affected parts is generally lowered, would seem to go in favour of this view. But no lesion of the ganglionic system has as yet been discovered. M. Bergeron believed that the paralysis of motor power was caused by the compression exercised on the muscular fibres by the hypertrophied connective tissue; but, unfortunately for this view, the paralysis does not always proceed side by side with the hypertrophy, but many muscles may be paralyzed without having their volume at all increased.

In fact, the pathology of this disease is extremely obscure, and must for the present be left unsettled. As to the age at which this disease may commence, none of the cases recorded by Dr Duchenne commenced later than 10 years of age. The case which I have related commenced at 26, and thus forms a rather important contribution to the history of the malady.

In many of these cases there has been no trace of an hereditary taint, while in a few it has attacked two or more members of the same family.

As to the prognosis, Dr Duchenne states, that if observed and treated in the *first* stage—that is, in the period of weakness, before the pseudo-hypertrophy has commenced—it may be cured. Indeed, he records two cases thus arrested or cured. But if it has gone beyond this stage, and the muscles are already enlarged, it is beyond the reach of curative measures. At least, none of the cases thus advanced have at all improved, but have gone on slowly but surely to the end.

The only treatment which has been found to be of the least use, in even the first stage of the disease, is local Faradization of the affected muscles; and it was by persevering in this treatment that two cases were cured by Duchenne. Along with this, however, he used iodide of potassium, or certain medicines supposed to be of use in the diseases of the nervous centres, such as nitrate of silver or the preparations of phosphorus. But used alone, these internal remedies, as well as strychnine and ergot, with baths, frictions and blisters, cauterizations in the lumbar regions, and hydropathy, were all found useless.

## ARTICLE IV.—*On the Causes of Endemic Fever in Lower Bengal, the Influence of Canal Irrigation in their Production in Upper India, and the Means of Prevention to be employed.* By J. SUTHERLAND, M.D., D.I.G., etc.

RECENT newspapers from Bengal report the great prevalence of malarious fever in the lower part of that province, particularly in the district on both banks of the Hoogly; and it is understood that the occurrence is one that is at present occupying the attention of the Bengal and supreme Government. Under these circumstances, as I have given considerable attention to the subject, and have lately visited the localities where the epidemic is now raging, I trust that a few observations regarding the prevailing fever may be of use in enabling the authorities, both in this country and in India, to form a correct opinion of the nature of the epidemic, and of the means to be adopted to arrest its progress.

2. The frequent occurrence and great fatality of the fever of late years have begun to have a serious effect on the average longevity and physical wellbeing of the people; nor is the moral effect of such a calamity to be disregarded. The question naturally occurs to the native mind, What good have the English done to us? Have

not some of their boasted improvements resulted in the physical, if not moral, deterioration of the people in the localities where their most magnificent works exist? Has not the railroad obstructed the natural drainage of the country, and converted some previously healthy localities into unhealthy and deadly hotbeds of malarious fever? Have not our canals created, wherever the irrigation system has been carried out, a deadly miasmatic atmosphere, which has destroyed as many lives as would have been lost by the casual famines which swept over those districts? The answer to such statements must be, that there is much truth in them. Imperfect drainage, in consequence of our great public works, has been by no means an unfrequent occurrence; they have produced a degree of dampness favourable for the production of malaria,—a result that might have been anticipated from our previous knowledge of the conditions under which malarious poisons are generated. But it may be said, that in the portions of Lower Bengal where epidemic fevers are most rife, there have been no great public works to obstruct the drainage—for example, at Jessore and portions of Nuddea. But this merely shows that there are other important agencies at work; and the chief of these is the silting up of rivers and lakes in the delta of the Ganges and other places. Examples of this have come under my own observation in various districts, in Upper and Lower Bengal, Hoogly, Nuddea or Jessore, and Rungpore.

3. There can be no doubt that the deposits brought down by the Ganges and its tributaries on their way towards the sea are gradually filling up large low-lying tracts of country and river-channels that will remain for a considerable time in the condition of a malaria-producing swamp,—this is the state of many tracts in Lower Bengal at present. Long before the British Government obtained possession of the country, the deposit referred to was going on, but the silting up had not attained that stage where the causes productive of endemic fever had acquired their greatest intensity.

4. One of the effects of the silting up of rivers in Lower Bengal is the extension of the flood-line in the inundations taking place in the rainy season. It is obvious that the more shallow the bed of a river becomes, the greater will be the area over which the water will spread during a flood, and the larger the quantity left to stagnate on the low grounds which it has inundated.

5. The means which we have adopted to facilitate intercourse—the construction of raised roads passable at all seasons—have in some cases doubtless disturbed the natural drainage of the country, and left water to stagnate in places which were formerly comparatively dry and free from swamps.

6. Should any one doubt the power of causes similar in all respects to those mentioned to produce such disastrous results, a reference to a Report made to the Government of India in 1846, by a Committee, of which Colonel W. E. Baker, of the Bengal

Engineers, was President, will afford a convincing proof of the fact. The members of the Committee travelled over 1400 miles of country irrigated by the Delhi Canal, etc., and inspected all the towns and villages in any way affected by canal irrigation; and they found everywhere unequivocal evidence that wherever the subsoil had been rendered excessively damp, there malarious fevers were rife; in dry localities, there was little or no spleen disease,—for this was the test used by Dr Dempster, the medical member of the Committee, to prove the existence of malarious fever.

7. In a paper read before the National Association for the Promotion of Social Science, in 1861, this officer states that "Canal irrigation carried on in the North-West Provinces of the Bengal Presidency, under certain unfavourable conditions, is uniformly productive of endemic disease, which from time to time assumes the form of widespread and often fatal epidemics."

8. The great prevalence of fever in the Burdwan and Nuddea districts in 1861–62 led the Government of Bengal to appoint a Committee to examine and report on the epidemic; and Dr Elliot, the Civil Surgeon of Hoogly, furnished in the following year an able Report on the nature of the fever, and of the state of the towns and villages in which it was prevalent. On a consideration of all the subject, I am now of opinion that too much weight was assigned to the condition of the affected localities as regards cleanliness, and that probably too little importance was attributed to the drainage and the state of the dampness of the towns and villages and places in their neighbourhood. In this respect, the conclusions arrived at were very different from those of Dr Dempster; the latter attributed the great prevalence of the fever in irrigated districts to a soil made artificially damp with canal water. Dr Elliot, on the other hand, was of opinion that the Nuddea epidemic was caused by bad conservancy, by the foul state of the tanks in the towns and villages and the presence of organic impurities, obstructed circulation of air, caused by jungle growing up all round the habitations of the people, and by their filthy habits.

9. In a question of this nature, a knowledge of the general laws that govern the production of epidemic disease will come to our aid. The fever of the Nuddea district was certainly not of the character of typhus or typhoid fever; it was a disease of an intermittent or remittent type, and such as we know from experience is usually caused by the emanations from marshy soils. Even if the disease was occasionally contagious, this would not invalidate the proof that the cause was marshy effluvia; fevers of the continued type, typhus and typhoid, are unquestionably associated with organic impurities, and originate from causes in which animal excreta and other similar agencies, entirely different from the emanations of a swampy soil, play the principal part. Dr Dempster was therefore, I think, right in assigning the prevalence of the fatal endemic fever to causes entirely independent of the sewage

and filth accumulated in towns, the foul water in tanks, or bad conservancy.

10. Having stated my views regarding the cause of the prevailing epidemic, or rather endemic—for the latter is the correct term to apply to a fever strictly confined to a limited locality, and produced by causes existing only within the space where it at present prevails, the disease having apparently no power to travel into any district in which the moist condition of the soil is wanting—I will now only further observe, that a fever similar in all respects to the endemic now raging on both banks of the Hoogly began in Jessore in 1824–25, and gradually extended itself to districts further west; but it is to be recollected that these districts bear a striking similarity to one another as regards drainage, subsoil, water, and general physical condition; and that the endemic, having exhausted itself in one locality by carrying off the most susceptible, would naturally progress into districts where it found fresh and predisposed victims, and all the physical conditions necessary for its spread. Contagion in such cases, it is well known, had no share in the propagation of the disease, although the fever might, under favourable circumstances, attack in this way a few persons who had been much exposed to the noxious emanations of the sick in confined and badly-ventilated huts or houses; such seizures, I believe, occasionally took place, but this was not the way in which the fever progressed.

11. Having considered the causes of the generation of the disease, I shall now proceed to point out the means of averting future outbreaks of the endemic fever. The natives, with some justice I think, complain that we have assigned too much importance to jungle and rank vegetation in tanks, and local impurities in their towns and villages. They have been annoyed by our cutting down more of their trees than they liked, and by our harassing them by sanitary measures which they do not see produce any salutary result as regards the prevailing fever. It is evident to them that the cause of the endemic lies deeper, and in this opinion they are probably right. If the disease we had to combat was cholera or typhus, no doubt the sanitary measures referred to would have had the best result; but the fever is the effect of a marsh poison, and to remedy this we must carefully attend to the conditions under which it is produced.

12. I am of opinion that the drainage of the country demands our chief attention, if the evils complained of are to be removed; much might be done by carefully executed lines of drains, and by greater attention to the watershed, by avoiding most carefully the throwing back of any current of water, so as to cause its stagnation in places formerly dry.

13. Wherever roads have been carried through a district, great care should be taken to find if they have not acted as artificial dams, and held back water seeking a lower level; culverts should be

constructed in all such cases. Along the line of railway I do not doubt that the necessity for a greater number of such passages for the free flow of water is greatly required, and the partial destruction of some railway bridges by floods in Bengal prove that this was not sufficiently attended to; but when all this is done, there will still remain much swampy and ill-drained land. And for this there is another remedy, which I am not aware has ever been brought to the notice of the Government of India—that is, the digging of deep tanks in all swampy or marshy localities at or near a town or village. From the earth so obtained the surrounding swamp would be raised till it became comparatively high ground; into these tanks all the drainage would flow. They should be of considerable depth, and drains should be carried into them in all directions.

14. That collections of pure water, although of considerable extent in any locality, are not *per se* injurious to health or productive of malaria, is a proposition to which all medical men and others who have given attention to the subject will readily assent. It is only when these artificial reservoirs (tanks) receive sewage or other impurities, or are allowed to be overrun with rank aquatic vegetation, that they become sources of disease. A swamp reclaimed in the way I have proposed would form arable land of a peculiarly rich and productive character; and in any such locality where the experiment is carried out of digging tanks in the marshy ground, I do not doubt that the result will be a great improvement on the general health of the residents, if the tanks are dug so deep that they will never be dried up in the hot season.

15. If it is objected that digging tanks in marshy land is too expensive for general adoption, I reply that it has not been tried,—that the value of land so reclaimed in certain localities, near towns especially, would be so enhanced as to pay a considerable part of the expense of the excavation.

16. The utility of tanks, as a means of improving the sanitary condition of a town or district, is no mere theory; places like the civil station of Burdwan, in Lower Bengal, formed out of rice-fields, owe their comparative salubrity, in my opinion, to the number of tanks they contain.

17. When I visited Kishnagur in Bengal, on a professional tour of inspection in 1865, I found the inhabitants of one of the suburbs, Buriparah, suffering to an intense degree from malarious fever; the ancient but now silted-up bed of a river bounded the locality to windward. By the advice of the medical officer of the station, tanks were dug in this swamp; the general level of the marsh was raised by the earth obtained in carrying out the excavation. Before this had been done, the suburb, which contained 8000 inhabitants, had lost 1000 by death, 5000 had left the place, and only 2000 remained, and the majority of these were in an extremely sickly condition. I again visited the station in November 1866, a

year after the tanks were dug, and found that the place had become comparatively healthy, and it was no longer necessary to keep up a dispensary, which had been established by Government on account of the prevalence of fever.

18. Several stations in Bengal, Jessore, Rungpore, etc., are so extremely malarious that few of the residents, Europeans or natives, escape the fever. To render such places healthy would be an incalculable boon to the unfortunate civil officers who have to reside there; they or their families are often prostrated by malarious poison, which it takes years to eradicate—in some cases even when the affected persons leave the district, and are sent to Europe for change of air.

19. In estimating the effect of obstructed drainage in causing endemic fever in Lower Bengal, it should be recollectd that the whole district is formed by sedimentary deposit; everywhere in the delta of the Ganges the superficial surface soil, generally about ten feet in depth, overlies a bed of clay, which is nearly impermeable to water. Dr M'Clelland, in his "Medical Topography of Bengal," states that without this surface-soil it would be a swamp, without the clay it would be a desert.

20. The Government of India are about to carry out canal irrigation to an extent hithertò unknown in India; but if great care is not taken the scheme may prove a perilous gift, for while it protects the inhabitants from absolute want in the event of the failure of the periodical rain, it may destroy their health, and leave them victims of malarious fever, diseased and wretched, hopelessly infirm, to die early, and to leave a weak and debilitated progeny to suffer from the means that was intended to be a benefit and a boon to their country. Such calamities can only be avoided in irrigated districts by carefully regulating the supply of water, by preventing the subsoil from becoming permanently under the influence of excessive moisture, and by taking care to preserve the water-level in wells in the dry season some feet from the surface.

It is well known that in many places in the North-West Provinces water is found at a depth of more than 100 feet; to swamp such localities by saturating the subsoil to excess, bringing the water in wells near the surface, would be to convert a dry and healthy into a damp and fever-breeding district.

There is, I believe, no insuperable difficulty in carrying out canal irrigation in such a way that it will not seriously deteriorate the health of the people:. by care it may be productive of infinite good; by neglect it will certainly prove an evil greater than the one it was intended to remedy.

## ARTICLE V.—*Chronic Hepatic Fistula—Cure by Injection of Iodine.*

### By LAUCHLAN AITKEN, M.D.

(*Read before the Edinburgh Medico-Chirurgical Society, 6th April* 1870.)

ARCHIBALD BUCHANAN, æt. 37, belonging to the Public Works Department in India, applied to me about the end of July last for the treatment of a chronic hepatic fistula. He was a tall powerful-looking and intelligent man, and gave me the following history of himself:—About eleven years ago he was sent to India with his regiment, and during the first six years of his residence there he enjoyed uninterrupted good health. At that time, owing to his steadiness and good conduct, he was promoted to the Public Works department; and during the remaining five years of his stay abroad, he was almost exclusively employed in engineering work. But with his promotion his health seemed to have failed, and from his constant exposure to miasmatic influence, he had frequent attacks of jungle fever; one of which, at least, was complicated by some form of inflammation in the lungs. During the same time he had several bilious attacks, as he calls them, accompanied by pain in the right hypochondrium. One day, in the month of November 1867, he had been completely exhausted by very heavy work in the sun, and imprudently lay down and fell asleep in a draught to which his right side was exposed. Next morning, on awaking, he felt very severe pain in the right hypochondrium, and, being quite unable to go to work, he sent for the doctor, who blistered him four times in succession, but without relief. He had no medicine to take, so far as he can recollect, except some effervescing draught. The pain remained acute for some six weeks, and a swelling which had appeared about a week after the attack in the right hypochondrium shifted round more to the epigastrium, where, eventually, a protrusion appearing, it was poulticed, and opened of itself. The opening, however, was enlarged by a bistoury, and about half a pint of thick matter and blood escaped. After allowing the matter to flow for a few days, the medical man began to inject the abscess with nitrate of silver, the exact strength of which, however, the patient did not know. As the discharge, instead of diminishing, increased under this treatment to from ten to twelve ounces of matter daily, the injection of iodine was substituted, but it proved equally useless, as also did port wine, which was subsequently tried. This treatment by injection was continued in all for nearly three months, the injections being made as frequently as from two to four times every day.

As the patient seemed to be rather losing than gaining ground, he was sent, in April 1868, to a hill station, where the physician stopped the injections, and used instead strong iodine liniment externally, giving him at the same time some acid to take, with purgative medicine every second day. He improved in general health, and the discharge diminished in quantity, but coincidently with this decrease in the discharge, the pain increased. He remained at the hill station nearly six months, and was sent home to England, and entered the Netley Hospital in December 1868. Here he was at first treated with tonics only, but as he did not improve much, the abscess was eventually injected with solutions of carbolic acid, of the strength of 1 to 200, but afterwards of 1 to 150. About six injections in all were made at Netley.

He left that hospital in May no better than when he entered it, and came to Edinburgh, his native place, where at first he abstained from all treatment, but at the end of July he came to me. The opening, which was immediately below the xiphoid cartilage, and about 3½ inches above the umbilicus, admitted easily the end of a No. 10 bougie. It had everted edges, and from the fistula trickled constantly a thick slimy mixture of pus and bile, especially abundant at the close of a long expiration. A probe penetrated about 3½ inches into the liver substance, and that organ was enlarged, measuring nearly five inches, in a vertical line from the right nipple. He had a sallow complexion and yellow conjunctivæ, but appeared to enjoy fair health apart from the weakening effect of the discharge.

On the first occasion of my seeing him, I injected a couple of drachms of the tincture of iodine (B. P.) into the fistula; and as the discharge diminished greatly, the same injection was repeated after an interval of three days, and again on the fifth day after that. The day following the last injection, I left for the Continent, and did not see him for more than a month. He then came back, and the improvement in his appearance was so marked as very agreeably to surprise me. Two days after the last or third injection, he told me, the discharge ceased altogether, and the wound entirely closed a day or two subsequently. He now eats and sleeps well, all his functions are healthy, and he has no pain on pressure over any part of the liver. There was a firm cicatrix at the point where the fistula had opened.

A month afterwards he left for India in as good health as he had ever enjoyed.

I record this case without any comment, and simply as an instructive example of the rapid and almost surprising success of an old and approved method of treatment after the failure of many remedies.

## ARTICLE VI.—*Case of Tuberculosis of the Kidneys.* By T. W. M'DOWALL, M.D. Edin., Assistant Medical Officer, Inverness District Asylum.

THIS case appears worthy of record, as affording an example of unusually extensive tubercular affection of the kidneys, and as illustrating in a marked manner the masking of the symptoms of organic disease when occurring in an insane patient.

The fact that lunatics are specially liable to tubercular affections has been recognised for many years, but even among them tubercular affection of the kidneys appears to be a much rarer disease than might have been expected. In an elaborate paper on "Tuberculosis and Insanity," which appeared in the *Journal of Mental Science*, 1863, Dr T. S. Clouston gives several tables which illustrate the unfortunate frequency of tubercular affections among the insane, and which prove that, when the deposit occurred in the lungs, in a large number of cases, no symptoms existed during life indicating such a disease. His statements are the more trustworthy, as they are founded on post-mortem examinations; and in a table he shows that in 463 cases dying in the Royal Edinburgh Asylum, 282 were found tubercular, this being at the rate of 60·9 per cent. He makes no special mention of the morbid deposit occurring in the kidney. This subject is further illustrated by a table of results in 314 autopsies, drawn up by Dr R. Fischer, and quoted by Dr Clouston. From this table it would appear that in 6 cases only, out of the 314, did there exist tubercular disease of the abdominal organs; and it may be fairly surmised that several, if not the majority of these 6, were examples of tuberculosis of the mesenteric glands—a disease to which the insane are, comparatively speaking, rather liable.

As illustrating the latency of symptoms, Dr Clouston gives the results of his investigations in a tabulated form, and remarks—"In about one-fourth of all the cases the symptoms of phthisis appeared at periods under three months before death; in one-fifth of them the period when they manifested themselves was between three and six months. In only about one-tenth of all the cases did symptoms of tuberculosis show themselves more than a year before death." His figures prove that "in one-third of the men, and in a little more than a fourth of the women, the tuberculosis manifested itself by no symptom during life." Surgical diseases also frequently run through their whole course without producing the slightest constitutional disturbance, in illustration of which I have the notes of case of a female lunatic who suffered from a very large carbuncle on her back, yet she worked as usual, was cheerful and talkative, had an excellent appetite, and presented the usual signs of robust health; and the fact that she was labouring under such a painful disease was only discovered when she was undressed for the bath. Facts like these go far to establish the idea that lunatics do not differ more in their mental condition from sane persons than they do in their bodily constitution; and should go far in assisting to convince medical men, accustomed only to practice among sane patients, that it is quite possible for persons, mentally affected, to labour under extensive injuries, such as fractured ribs, and yet present not the slightest symptom to indicate such injury, but rather appear to be in the best possible physical health.

The notes of the present case I have extracted from the case-books, but have curtailed them as much as possible without omitting any fact of importance.[1]

D. G. was admitted in June 1865, labouring under dementia of three years' duration. His physical health appeared fair, careful examination did not discover any physical disease whatever, and his mental condition was characterized by idleness, listlessness, slight irritability, and almost perfect silence. A few weeks after admission he laboured under a very transient attack of hæmaturia, and some months afterwards he was confined to bed for a fortnight on account of a scrofulous abscess in one of his testicles. From this affection he speedily recovered, and under the use of liberal diet, cod-liver oil, tonics, etc., his bodily health became much improved. Simultaneously with this physical improvement he became industrious, and until the end of September 1869 he was employed regularly as assistant fireman, and during this period of four years there was no return of the hæmaturia and no indication of physical disease, and the patient's aspect indicated so strongly a good state of health that the attack of hæmaturia was entirely forgot. In September 1869, his bodily health continuing as good as ever, he began to remain in the house instead of attending to his work, and he speedily relapsed into his former condition of idleness and listlessness. In the beginning of January 1870 he was reported as not taking his food so well, and on examination it was found that he appeared to labour under an attack of aggravated dyspepsia—his tongue being very much furred, moist, breath foul, and slight pain at pit of stomach, but no other morbid symptom, his pulse 80; temp. 98·4. Various attempts were made to improve his condition, but with very little benefit; the dyspeptic symptoms remaining almost *in statu quo*, and he in the meantime becoming decidedly emaciated. He was under constant observation, but was never observed to cough, to expectorate, to pass urine frequently, nor did he ever complain of any pain, although frequently questioned, except the one in the epigastrium. His chest was examined frequently, the presence of tubercle being suspected, and this suspicion was strengthened by the existence of a decidedly harsh respiratory murmur at both apices anteriorly, and by increased vocal resonance; but at no part of the chest could crepitation be discovered; there was no sub-clavicular flattening, neither was the respiration hurried or otherwise abnormal. His pulse ranged between 78 and 84, and was rather feeble. Thermometric examinations, taken at various times until three days of the patient's death, never gave a higher figure than 98·6. For some days before the 29th January, there were indications of intense intestinal derangement. His tongue was swollen, somewhat like semi-putrid flesh; the cuticle became detached in large flakes; his breath was foul, bowels constipated, and appetite almost destroyed. He was ordered an aloetic

[1] I have to thank Dr Aitken for permission to make use of this case.

purgative, and in a few hours after taking it he became much worse —he was violently purged, suffered greatly from tenesmus, hiccough, irritability of the stomach, and inability to retain the smallest quantity of solid food. Pulse 100, soft, and very feeble. Stimulants, beef-tea, ice, etc., were tried for the relief of the symptoms, but without any benefit. For twenty-four hours previous to death he was cold, collapsed, and the face was ghastly; but he was quite conscious, and even answered questions, though in an incoherent manner. He died on 1st February at 4 A.M.

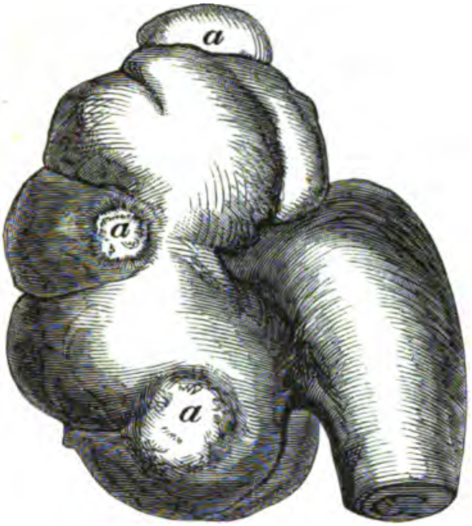
*Post-mortem examination,* twenty-four hours after death.—The facts observed are stated as briefly as possible, except in the description of the kidneys, which is given more in detail.

*Thorax.*—All the cavities of the heart contained blood, and in the ventricles were found decolorized clots firmly attached to the ventricular walls and prolonged into the aorta and pulmonary artery. Valves competent. Weight 8¼ oz. *Lungs.*—In both lungs there was considerable tubercular deposit at the apex, and in two or three places in each, softening had occurred, forming cavities in each case only large enough to contain a field bean. Throughout both lungs there existed a miliary tubercular deposit in all stages. Right lung 20¼ oz., left 25 oz.

*Abdomen.*—Stomach contained about 10 oz. of a bilious fluid, and the mucous coat was slightly congested in several places. *Liver* normal; weighed 40½ oz. The gall-bladder contained a small quantity of bile, which was very pale. *Intestines.*—The mucous membrane was almost universally greatly congested; in some places, indeed, enteritis appeared to exist. The intestinal contents were very dark, almost black. The large intestine presented no markedly abnormal appearances, and contained a few masses of fæces.

*Kidneys.*—*In situ* these organs presented a very peculiar appearance. The left appeared twice the size of the right, and both were distended as if about to burst at several spots. These spots existed at several places on the surface of the organs, and had the appearance of rounded projections, such as is seen in cystic disease of the kidney. That these irregularities did not depend upon the presence of cysts was at once evident, as upon the removal of the finger after applying pressure the parts retained the pits so produced. The pelvis of each kidney was similarly distended, and the swelling was prolonged along each ureter about two inches. In Fig. 1 there is a rough sketch of the right kidney, the smaller of the two (on the scale of two-thirds the natural size), in which the rounded projections are marked *a*. Considerable difficulty was experienced in removing the organs, as the slightest pressure altered their shape; but by careful dissection they were at length removed for further examination. In cutting across the ureter in each case, the blade of the knife became covered with the dirty white substance to be again referred to, and this substance could be squeezed out at the cut end of the ureter by very moderate pressure applied to the kidney.

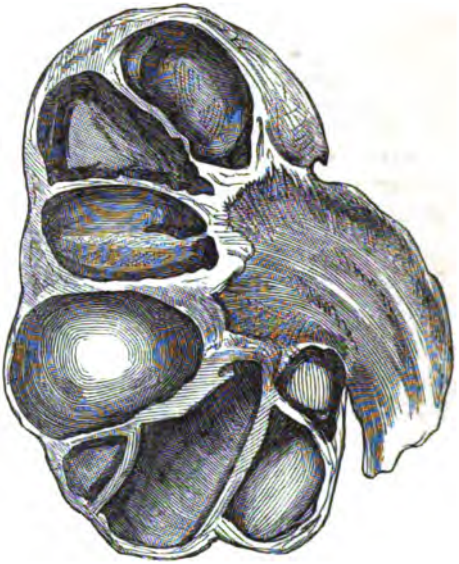
Fig. 1.



*Right Kidney.*—Anterior surface, ¾ natural size.

*Right Kidney.*—Weighed 5½ oz. On making a section in the usual manner, absolutely nothing was visible but the tubercular matter already mentioned. This was carefully removed, and it was then found that, besides filling the pelvis, it occupied the position of the pyramids, and that the whole kidney was reduced in appearance to the condition of a multilocular cyst, the proper renal structure being universally destroyed, and the connective tissue hypertrophied, and forming the walls and septa of the cysts. The tubercular matter occupying the position of the pyramids was of the consistence of putty, and could, with but little difficulty, be enucleated. The surfaces of the various loculi were white, and presented a rough appearance, in some spots there being a somewhat honeycombed condition. On attempting to strip off the capsule, it was found to be incorporated with the surface of the kidney almost universally. This unity of substance was particularly marked where the rounded projections occurred; at these spots the structures could not be separated, and the application of any force caused the formation of a rent through the wall of the so-called cyst. When the whole of the tubercular matter was washed from the kidney, it was found that the organ, including the capsule, weighed only 1½ oz. In Fig. 2 the arrangement of the tubercular masses is shown; at least, the cavities are represented in which the masses lay.

Fig. 2.



*Right Kidney.*—Section showing the cavities in which tubercular matter was contained, ⅔ natural size.

*Left Kidney.*—In some respects it presented a striking contrast to the right, although affected by the same morbid deposit. It was fully 6 inches long, and weighed 12½ oz. On its surface there were several rounded projections, such as were found in the right kidney, and figured in the sketch, but they were more numerous and of larger size. A section being made, a condition very similar to that in the other kidney was found, but the disorganization was not so universal, although, where it did exist, the destruction of original tissue had proceeded quite as far. The tubercular matter filled the pelvis, and greatly distended it. Throughout the organ, collections of the same substance existed, of various sizes, occupying the position of several of the pyramids, and also encroaching on the medullary portion; and in three instances these collections extended quite to the capsule, elevating it into the rounded forms described as existing on the surface. The renal structure unaffected by the morbid process was greatly hypertrophied, weighing 11 oz., thus forming a marked contrast to the right, which weighed only 1½ oz.

These, then, are briefly the history and post-mortem appearances, and they seem to warrant the following statements:—

1. It is probable that the hæmaturia, which occurred in 1865, indicated the commencement of the kidney affection.

2. The right kidney was probably the first to be affected; and as the disease advanced in it, the left kidney became hypertrophied to compensate for the destruction of renal texture in the former.

3. It may be impossible to fix the date of appearance of the tubercular deposit at the apices; but it is probable that the miliary deposit occurred during the last few weeks of the patient's life, when he suffered from the severe dyspeptic symptoms.

4. That in this case we have an illustration of the suppression of symptoms which sometimes occurs, but most frequently in the insane. To indicate the chronic tubercle in the lungs, there existed no cough, expectoration, etc.; and during the deposition of miliary tubercle, we had no deviation from the natural temperature of the body.

5. That a patient may be under observation for years labouring under tuberculosis of the kidneys, and yet appear in good health, be able to engage in active employment, and yet never exhibit any symptom such as to indicate the fact that he laboured under any physical disease whatever, nor, in fact, that he experienced any personal discomfort or distress.

## ARTICLE VII.—*On the Action of the Cobra Poison.* By J. FAYRER, M.D., F.R.S.E., C.S.I.; Surgeon, Bengal Army; Professor of Surgery in the Medical College of Bengal.

(*Continued from page* 1003.)

### SEVENTEENTH SERIES.

Present—Dr Fayrer and Mr Sceva.—31st July 1869.

EXPERIMENT No. 1.—Mr R——'s (of Jounpore) antidote, the powdered root or bark of a plant, name and family unknown, was tried to-day on a dog. The drug had been sent to me for the purpose, and was fresh and potent. Half of a powder, the quantity directed by Mr R——, was given, having been first carefully rubbed and mixed with about an ounce of water.

A pariah dog was bitten in the thigh by a cobra at 3.3 P.M., and was much excited by the bite. At 3.6 P.M., as symptoms of poisoning appeared, the first dose of the antidote was given, and was all swallowed. The dog was led about, and cold water dashed on its face and thorax, when it seemed drowsy. 3.8.—Lies down; very restless. 3.9.—Hurried breathing. 3.10.—Dog lies down; rises again, and runs about in a restless and excited manner. 3.12.—Restless and uneasy; head swings about as though it were giddy; breathing accelerated. 3.17.—It staggers as it is walked about; cold water sprinkled on its head and chest. 3.18.—The second dose given, that is, the other half of the powder, as directed. 3.20. —The dog is worse; cannot stand, staggers and reels when walked about, and falls over; convulsive movements of head and neck. 3.22.—Convulsed; pupils widely dilated. 3.24.—Dead, in twenty-one minutes.

The dog was not a large one, but it was healthy and vigorous: the instructions sent with the drug were carefully followed. The result is not favourable to the drug as an antidote in the canine race.

Experiment No. 2.—A dog had a ligature made of stout cord, *soaped* to make it knot tightly, thrown loosely round the forearm. It was then bitten by a cobra below the ligature, which was tightened as firmly as a man's strength could draw it. Immediately after the bite, a red-hot iron was introduced into the fang wounds, and the bitten part thoroughly cauterized, strong carbolic acid having first been well rubbed in. Bitten at 3.31 P.M. Ligature tightened within five seconds.

Carbolic acid and actual cautery applied at 3.33, that is, in two minutes after the bite, and one minute and fifty-five seconds after the ligature was tightened. The limb seemed to be completely strangulated; it became livid; blood oozed from the fang wounds, and the limb was all but paralyzed. There could be no doubt that the limb was thoroughly strangulated, or that the bitten parts were well cauterized. 3.36.—Notwithstanding all the above precautions, the dog is already much affected by the poison; is lying prone, unable to rise or to walk; the breathing hurried; and convulsive movements occurring occasionally. 3.40.—Convulsed. 3.41. —Dying. 3.42.—Dead, in twenty-one minutes.

There was at the most an interval of five seconds between the cobra's bite and the tightening of the ligature, which was not afterwards relaxed. This experiment clearly proves that the poison is taken into the circulation very rapidly; certainly five seconds did not elapse between the bite and the application of the ligature, which had been previously thrown loosely round the limb, in order that no time might be lost in tightening it after the bite, and yet the dog (it was a small one) died of the poison in twenty-one minutes.

During that very brief interval sufficient poison entered the circulation to destroy life. It is possible that more may have entered after the ligature was tightened, but the quantity must have been very minute, as the ligature was very tight. In an ordinary snake-bite it is difficult to conceive that a ligature could be applied more speedily than in the case of this dog. So that even this method of treatment, rational as it certainly is, can only be regarded as of doubtful benefit.

I should note—and it is a subject, I believe, that I have not alluded to before in other experiments—that the rigor mortis took place in about 1½ hour after death in these two dogs. The blood coagulated after death.

EXPERIMENT No. 3.—A fowl had a ligature placed on the thigh loosely: it was bitten by a cobra at 3.47. The ligature was tightened at the same time that the snake bit; before its fangs were withdrawn, the ligature was thoroughly tied, so tight that the limb seemed completely strangulated, the part becoming livid and disabled. 3.50.—No sign of the poison taking effect; the fowl hops about on the sound leg. 3.52.—Actual cautery applied to the fang punctures, which were bleeding freely venous blood from the congested limb, and the wounded parts surrounding were thoroughly disorganized.

The ligature was then divided; the fowl, being placed on the ground, ran about; the ligatured limb still paralyzed. 3.54.—Fowl crouching, but rises and runs about when disturbed. 3.55.—Looks drowsy; is crouching, and begins to hang its head, closing the eyes. 3.57.—Head drooping, beak resting on the ground. 3.58.—Fallen over on its side, rises with a convulsive movement, and falls again. 4 P.M.—Is unable to stand or walk. 4.4.—Convulsive movements. 4.11.—Dead, in twenty-four minutes. Blood coagulated after death, when removed from the great vessels.

This experiment, more than ever, proves the subtle and deadly nature of the poison. The ligature in this case prevented the entry of the poison into the circulation, which was evidently retained in the congested part of the limb below the ligature. Carbolic acid and the actual cautery applied to the wounds, most thoroughly, failed to destroy it. Yet no sooner was the ligature relaxed than the poison entered the circulation, weak and altered as it must have been after the severe pressure of the ligature, and rapidly killed the bird. This proves that there is danger after removal of the ligature when it has been most effectually applied. The poison spreads itself by diffusion throughout the juice of the strangulated part; so that nothing short of destruction or removal of the whole of that part seems to offer a hope of subsequent escape from toxic absorption.

With reference to the application of a ligature above the bitten part, I would here remark that it is almost physically impossible with the power of one pair of hands so to tighten a cord round a dog's leg as thoroughly to strangulate the limb. The experiments seem to prove this, but also to show that it is possible completely to arrest the circulation through a fowl's leg in this manner.

With tourniquets it might be done, no doubt, and a man's arm or leg, certainly his toe or finger, might be so strangulated; but as ordinary snake-bites do not occur where any tourniquets, other than sticks and cords or the like, are forthcoming, the desideratum is to obtain the most perfect compression of the limb, in the simplest way possible, sufficient at all events to prevent immediate entry of the poison through the circulation; and this may be done with an ordinary cord or strip of cloth, twisted with the common stick tourniquet, and the fullest extent that the strength of the hands is able to twist it. But it must be borne in mind that this compression only extends to a certain depth, and that deeper the circulation still goes on; with this the poison retained by the ligature in the partially strangulated portion will soon communicate by diffusion, and symptoms of poisoning will supervene. In such a case we may fairly hope that the amount of poison entering the blood has been so far limited as not to be fatal, and that we may therefore be able to help the sufferer through the troubles caused by the reduced dose of the poison. But it is obvious that the urgent necessity is for the application of some agent that will equally diffuse itself, and neutralize or destroy the poison whilst yet retained and only partially diffused through the strangulated part. In this, as far I can understand it, lies the only hope of safety in a real cobra-bite. Carbolic acid or other allied substances would probably be useful. But it is obvious that the success of this, or indeed of any mode of treatment, lies in the promptitude and tension with which the ligature is tied, and the decomposing agent applied.

Dr W. J. Palmer, Professor of Chemistry, was present also.

EXPERIMENT No. 4.—A fowl had a ligature thoroughly tightened round the thigh, and was then bitten below it by a cobra at 4.7 P.M. 4.19.—No effect of the poison visible. 4.22.—Breathing rather hurried, but otherwise seems unaffected. 4.30.—Begins to show signs of the effects of the poison, nods its head drowsily, rests its beak on the ground; it is evidently affected. 4.35.—Much the same; thirty drops of the liquor ammoniæ injected in three doses with the hypodermic syringe. 4.37.—Fowl is drooping fast, cannot move. 4.41.—Convulsed. 4.44.—Lies unconscious, but convulsed. 4.50.—Dead.

In this case the ligature, which consisted of a cord soaped to make it run easily and knot firmly, was tied round a fowl's thigh, from which the feathers had been stripped, with the greatest amount of tension that a man's hands could exert. The part below the ligature became livid, and the limb paralyzed. In this condition it was bitten at 4.7 P.M. The ligature was never relaxed, and certainly did not slip, yet at 4.30, perhaps earlier, that is, in twenty-three minutes, it began to show that the poison had, notwithstanding the ligature, found entry into the circulation. Its death, twenty-one minutes later, proved that sufficient poison had entered to destroy life, and also proves, I think, that it is almost beyond our power to keep it out. The question is, supposing the strangulation of the limbs to have been complete, How did the poison enter? It must have passed the barrier of the ligature. How did it do so? I can only explain it by supposing that, tense as it was, it was not sufficiently constricted to prevent some diffusion of the poisonous fluids through the compressed tissues, and that, in the space of twenty-three minutes, enough found its way in to destroy life.

From this experiment, I think, we may fairly deduce the amount of safety that may be expected from the ligature. That it retards the entry of the poison is abundantly proved, and that it gives time therefore to operate on the retarded venom, is also obvious. But it is equally evident, so subtle is this poison, and such is the power of diffusion, that nothing short of the most rapid and effective application of the ligature, and the immediately subsequent application of some decomposing agent, can, in a *bonâ fide* cobra-bite, offer any hope of safety.

EXPERIMENT No. 5.—A fowl was deeply bitten in the thigh by a daboia at 4.31. The snake had been in confinement for some time, but it was vigorous and vicious, and plunged its fangs deeply into the bird's thigh, drawing blood. 4.36.—Not affected. 5 P.M.—Not affected. No symptoms of poisoning occurred, and the fowl was alive and well on the 2d of August.

This experiment is a most instructive one, and proves that a poisonous snake may bite without poisoning. It is not in the least probable that this daboia was altogether exhausted; for although in captivity, it had been at rest for many days, and had not exhausted its poison by biting; another daboia that had been six months in captivity, and had eaten nothing during that period, killed a fowl rapidly by one bite. It furnishes an explanation of some of the so-called recoveries from snake-bite, in which, when the snake has been seen and the punctures of its fangs are visible, the patient recovers from the mental alarm and prostration after the administration of one of the supposed antidotes. That such alarm does cause temporary physical as well as mental prostration I have had proof in the following case:—

Some time ago, on visiting the hospital one morning, I was told that a man had been admitted during the night suffering from a snake-bite, and that he was very low. I found him in a state of great prostration; he was hardly able to speak, and seemed to be in a state of great depression. He and his friends said that during the night, in going into his hut, a snake bit him in the foot; that he was much alarmed, and rapidly passed into a state of insensibility when they brought him to the hospital. They and he considered that he was dying, and evidently regarded his condition as hopeless. He was in fact in that condition not unfrequently described, from which the sufferer has been snatched by the timely administration of an antidote. On asking for a description of the snake, they said they had caught it, and had brought it with them in a bottle. The bottle was produced, and the snake turned out to be a small innocent Lycodon. It was alive, though somewhat injured by the treatment it had received. On explaining to the man and his friends that it was harmless, and with some difficulty making them believe it, the symptoms of poisoning rapidly disappeared, and he left the hospital as well as ever he was in his life in a few hours. Had no snake been found, and had an antidote been given, who would have been prepared to dispute its efficacy? I am sorry to destroy popular and favourite illusions, when they are harmless, but in a matter of this kind it is well that the truth should be known.

EXPERIMENT No. 6.—A fowl was bitten by a daboia in the thigh at 4.49 P.M. The snake has been over six months in captivity, during which time it has steadily refused to take food or water. It was active, vigorous, and vicious; it plunged its fangs deeply into the fowl's thigh, and drew blood. In twenty seconds the bird was violently convulsed; in sixty more seconds it was dead.

Contrast this experiment with the preceding one, and I think it confirms what I said as to the occasional uncertainty of a snake-bite. These two daboias were both old, that is to say, old in captivity. They were both, notwithstanding, vigorous, and bit fiercely. In one case, no evil resulted from the bite; in the other, rapid death. The blood of the fowl was examined after death. Dark-coloured coagulated blood was found in one of the great vessels near the heart. In others, and in the cavities of the heart, it was fluid, and remained so after death. It is worthy of notice that in the mammals poisoned by the daboia the blood was found to be fluid, and to continue so after death. In birds it was sometimes coagulated. Could this be due to the rapidity with which life was extinguished in the bird?

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### EIGHTEENTH SERIES.

Present—Dr Fayrer; Dr W. Palmer, Professor of Chemistry; and Mr Sceva.—7th August 1869.

EXPERIMENT No. 1.—A pariah dog was bitten by a cobra (*Teturiah keautiah* of the snake-men) in the hind-leg at 3.5 P.M. At 3.8 P.M., thirty drops of liquor ammoniæ, sp. gr. ·959, diluted with three parts of water, were administered. 3.12.—Dog lying down, licking the wound; when walked about, limped on the bitten leg; breathing hurried. 3.15.—Thirty more drops given as before. 3.22.—Lying down; limbs twitching. 3.23.—Thirty more drops given. 3.24.—Convulsed; lying down; unable to rise. 3.25.—Dying; limbs convulsed; pupils widely dilated; tapetum lucidum very brilliant. Heart still beating, no respiratory movements. 3.26.—Pupils contracted again (this is a phenomenon I have not before observed). 3.28.—Another thirty drops of liquor ammoniæ administered. 3.29.—Heart still beating irregularly. 3.30.—Dead, in twenty-five minutes.

Ammonia has long been considered one of the most potent of all remedies in snake-bites. The object of this experiment was to test its value. The result is not encouraging.

EXPERIMENT No. 2.—Mr R——'s "antidote" was again put to the test. The powder was rubbed into a pulp mixed with water in the proportion directed; it was then administered to a dog at 3.31 P.M. The dog was then bitten by a cobra in the thigh. 3.35.—The dog is affected by the poison, looks scared, and limps in the bitten limb. 3.37.—Staggers, lies down; breathing hurried. 3.39.—Another dose administered. 3.43.—Limbs convulsed. 3.45.—Paralyzed; heart beating irregularly. 3.59.—Heart stills beats; no respiratory movements. 4 P.M.—Dead, in twenty-eight minutes. I am afraid the antidote must be regarded as inapplicable to the canine race.

EXPERIMENT No. 3.—Jugular vein of a pariah dog exposed at 3.42, and a diluted solution of liquor ammoniæ, sp. gr. ·959—one part to two parts water—to the extent of thirty drops, injected. No apparent inconvenience caused to the dog by the injection. At 3.43, the dog was bitten in the thigh by a cobra. 3.48.—Dog showing signs of the poison; thirty more drops, diluted in the same way with six of water, again injected into the jugular vein. Shortly after this, the dog began to turn round and round in the most restless manner; thirty more drops injected similiarly diluted in the other external jugular, as a large thrombus had formed in that part exposed. 4.10.—Dog convulsed. 4.12.—Cannot stand, limbs paralyzed. 4.13.—Violently convulsed all over. 4.20.—Dead, in thirty-seven minutes.

The cobra was not fresh in this case, and yet it killed in thirty-seven minutes. The injection of the diluted ammonia was not more satisfactory than that of the undiluted, as far as its immediate antidotal effects were concerned; but it would appear that the injection of diluted liquor ammoniæ into the jugular vein is not followed necessarily by convulsions or other violent constitutional disturbance.

EXPERIMENT No. 4.—Some of the blood of the dog killed by the cobra in the first experiment, where the ammonia was given, was removed from the body about three-quarters of an hour after death. It was found to be firmly coagulated, but some of the serum and part of the clot mixed with water, to the extent altogether of forty drops, were injected with the hypodermic syringe into a fowl's thigh: the actual quantity of blood thus used could not have been more than a few drops. The injection was made at 4.20 P.M. 4.35.—Slightly affected by the poison. 5 P.M.—Crouching, head drooping, appears giddy. 5.30.—Lying on one side; convulsive movements. 5.35.—Dead, in seventy-five minutes.

What can more forcibly illustrate the extraordinary virulence and potency of the poison than this experiment? A few drops of the blood of a dog poisoned by a cobra, diluted with water, injected into a fowl's thigh, killed the bird in seventy-five minutes. The quantity must have been excessively minute, but it proves how it retains its power, although diluted and mixed with the blood.

### NINETEENTH SERIES.

Present—Drs Fayrer, W. Palmer, and Mr Sceva.—
14th August 1869.

EXPERIMENT No. 1.—A gentleman residing in Rohtuck having forwarded to me the powdered root or some other part of a plant, name and family unknown, which he had found useful in the treatment of snake-bites, and having requested me to test its efficacy, the following experiment was made:—ʒi. of the powder was rubbed with six peppers into a pulp and mixed with water.

A pariah dog was then bitten by a cobra (variety *Kurris keautiah* of the snake-men) in the thigh at 3.13 P.M.; part of the antidote was then, according to Mr F.'s direction, rubbed into the punctures, and the remainder administered internally, immediately after the outward application. 3.18.—The dog is affected by the poison; he is restless, nauseated, making efforts to vomit; walks with a staggering gait. 3.22.—Limbs partially paralyzed. 3.23.—Convulsed, unable to rise. 3.25.—Lies perfectly motionless, muscles generally twitching. 3.26. —Dead, in thirteen minutes.

The drug had evidently no effect in retarding the action of the poison. The dog, which was a medium-sized animal, died even sooner than usual.

EXPERIMENT No. 2.—A Mahomedan hakeem, Mahomed Khan, presented himself with some medicine with which, he said, he had successfully treated several cases of snake-bite in men. It was a strong aromatic smelling powder, dissolved in water, but he could tell me no more than that it was a jungle root. He asked to be allowed to try it, and appeared quite confident of success. A very large and powerful pariah dog was then placed at his disposal, also a cobra, which was not fresh, having been in captivity for some time, and had bitten before. He had the dog bitten in the thigh by the cobra at 3.35 P.M. He was allowed to do, or direct to be done, whatever he liked. At 3.36 he administered a quantity of the drug, which was swallowed by the dog. 3.37.—The bitten leg is partially paralyzed. 3.45.—The dog is sluggish and lying down. 3.46.—A second dose administered. 3.48.—Hurried breathing. 3.50.—The dog is nauseated, and rejected some half-digested meat. 3.55.—Uneasy; hurried breathing. 4.2.—Lying down, panting, frothing at the mouth. 4.5.—Retching. 4.7.—Lying down; looks depressed, but quite intelligent. 4.15.—When roused staggers as he walks. 4.18.—Lies prone, with the legs outstretched. Has very little control over the hind-legs when roused. 4.20.—Another large dose of the drug administered by the hakeem. 4.21.—Limbs convulsed; unable to rise. 4.24.—Tries to rise; falls over. 4.26.—Convulsed. 4.32.—Is quite paralyzed; pupils widely dilated. 4.35.—Heart still beats; no respiratory movements. 4.40.—Pupils contracted again (I have observed this symptom in another dog just before death). 4.42.—Dead; pupils again dilated. Bitten at 3.35; dead at 4.42—in sixty-seven minutes.

The dog was a remarkably powerful and vigorous animal. The snake was not fresh, and yet the dog succumbed in one hour and seven minutes. The hakeem expressed much astonishment at the results; he evidently *believed* that his drug would prove an antidote. He said, in a somewhat depressed tone of voice, that he had other remedies. He was invited to put them to a similar test.

EXPERIMENT No. 3.—A very large and vigorous pariah dog was bitten in the marginal fold of integument between the thigh and abdomen by a cobra at 3.55 P.M. The part was immediately cut out with a bistoury, the places where the fangs had penetrated being completely removed. The instrument was at hand, and the operation was done at once. Two seconds, not more, might have intervened between the bites and the excision. At 4 P.M., some brandy was poured down the dog's throat. 4.6.—Another dose of brandy administered. 4.16.—He is excited, and the respiration is hurried, perhaps from the brandy. 4.25.—The dog is not yet affected by the poison. 4.33.—Much the same, the breathing rather hurried. 4.42.—No symptoms of poisoning except the hurried breathing, and that may be from excitement. 4.47.—More brandy given. 4.50. —No symptom of poisoning as yet. 5.10.—Vomited; shows symptoms of being poisoned. 5.15.—Vomited again. 5.30.—Restless, breathing hurriedly; abundant flow of saliva. 6 P.M.—Slight convulsions; breathing hurried. 6.30.—Dead. Bitten at 3.55; dead at 6.30—in two hours and thirty-five minutes.

Here again the extraordinary virulence of the poison is shown. The snake bit in a fold of skin which was immediately excised. Yet in the slight interval—it could not have been more than two seconds—enough of the poison had entered the circulation to cause death in two hours and thirty-five minutes, notwithstanding the free administration of brandy. The dog, too, was an unusually large and vigorous animal.

EXPERIMENT No. 4.—A fowl was bitten in the thigh by a cobra at 4.13 P.M. The part in which the fangs had lodged was immediately excised with a sharp scalpel. 4.17.—Fowl lying down, showing no signs of poisoning. 4.20.—Fowl rather drowsy, eyes closing, head drooping. 4.25.—Breathing hurried; drowsy. 4.28. —When roused can stand, but cannot walk, and falls over; gasping. 4.31.—Convulsed. 4.33.—Dead, in twenty-one minutes.

This again shows the extraordinary virulence of the poison. The entire mass of muscle into which the fangs were impressed was clearly cut away within three seconds after the bite, and yet poison sufficient had found entry to cause death. That death was much retarded there can be no doubt, for the fowl lived twenty-one minutes, instead of three or four, after being bitten. Slight as the encouragement is to be derived from such experiments as this, it yet points in the right direction in which we are to look for any rational treatment.

EXPERIMENT No. 5.—A fowl was bitten in the carpal extremity of one wing, in a thoroughly vascular part, by a cobra at 4.40 P.M. This was amputated at the carpal joint immediately the fangs were withdrawn. The scalpel was ready, and it was removed within three seconds of the completion of the bite. The amputation was about half an inch above the highest fang's mark. 4.48.—No symptom of poisoning, no bleeding from the wing. The fowl is running about quite indifferent to either poison or amputation so far. 4.55.—No symptom of poisoning as yet.

*15th August, Noon.*—The fowl is alive and well. In this case, the poison has evidently not entered the circulation, the excision having been in time to prevent it. These experiments all prove that the poison takes effect chiefly through the venous circulation, and that if excision be practised immediately and thoroughly, either the whole or part of it may be prevented from entering the circulation. No doubt, some of the poison finds its way into the circulation by diffusion from the centre of inoculation, and thus all may not be removed by even very free and very early excision. The natural deduction is, that the part should be cut out as rapidly and extensively as possible; otherwise, as in the cases of these animals, delay of a few seconds may prove fatal.

## ARTICLE VIII.—*Acute Muscular Atrophy.* By JOHN HADDON, M.A., M.D., Eccles.

*History.*—John B., aged 21, a gardener, states that his parents are both in good health, and that he does not recollect of having had any illness.

On Monday, 7th June 1869, he was in his usual health. At night he went to a marriage party, where he had some dancing. He went to bed at half-past twelve, and slept well. On the morning of 8th June, he arose and dressed himself without feeling anything unusual; but when half-way down stairs, he felt what he calls a stiffness in the right thigh. In the course of that week the stiffness extended into the left thigh, and down to the legs. At the same time he had a sharp pain, as if a knife were being thrust into his back, over the upper part of the sacrum. This pain was occasionally so severe that he was bent quite double, and could not raise himself when in bed.

On Monday, 14th June, he first felt pain in the arms, and in the little and ring fingers of both hands. For the space of about a day, these fingers were so contracted that he could not straighten them.

On 17th June, he felt a little better, and tried to work, but his symptoms increased, and he consulted me on the 18th of June, for what he called "rheumatism in the arms."

*Examination.*—He was of average height, a strong-looking young man, of a spare build, and without any colour in his face. I asked him to undress that I might see the parts complained of, and was struck with the peculiar manner in which he undid the buttons of his waistcoat. Having observed this motion before, I was led to examine his hands. I found the ball of his right thumb much atrophied, and that of the left affected, though in a less degree. The right deltoid, too, was soft and atrophied, and the left to a less extent.

On asking him to hold out his arms at right angles to his body, he could not do so; but with considerable exertion managed to elevate them to an angle of about 45°, where they hung as if the hands were too heavy. He said he had a prickling feeling in the deltoid and biceps, on both sides. He also had a sharp pain over the upper part of the sacrum, which was not increased on pressure; and he complained of slight stiffness in the thighs, without any noticeable change in the muscles.

On the application of galvanism, he felt it very slightly in the muscles of the ball of the right thumb, and in the right deltoid. In the corresponding parts on the left side, it was felt much more distinctly. The biceps on each side was equally sensitive.

On examining his eyes with the ophthalmoscope, the right optic disc was much redder, and less easily distinguished than the left, which appeared normal.

His eyesight was good, and in every other respect he was quite healthy.

*Treatment.*—The galvanism I applied as a diagnostic. I ordered him a dozen of pills, one of which he was to take three times daily, and each pill contained a quarter of a grain of nitrate of silver.

*Progress of the Case.—26th June.*—He returned to-day to say that he feels quite well. He has been working a little, and can put his arms in any position with great ease and force. I tried galvanism again, with the same result.

*3d July.*—To-day he says he feels the pain in his back again, and also in the left deltoid and biceps. The muscles remain the same with regard to galvanism. He was ordered another dozen of the pills.

*10th July.*—He reports himself quite well.

*4th September.*—He has been at work since last report. He feels quite well, and is much stouter. The muscles are affected in the same way as formerly by galvanism, and his eyes present the same ophthalmoscopic appearances.

*10th January* 1870.—He is still in excellent health, and regularly at work.

## ARTICLE IX.—*Case of Union of a Fractured Bone in a Paralyzed Limb.* By THOMAS ANNANDALE, F.R.S.E., Surgeon to the Edinburgh Royal Infirmary.

THE following brief note is an addition to the short list of recorded cases[1] in which perfect union of fractured bones has taken place, notwithstanding that the injured limbs were in a paralyzed condition. On the 14th October 1869, I was asked by Dr Menzies to visit R. L., æt. 65. The patient had been paraplegic for twenty years, and confined to bed all this time, with the exception of occasionally sitting up while his bed was being made. Yesterday, when being raised up for this purpose, he fell on the floor and injured his left leg. An examination showed that he had fractured the tibia and fibula about three inches above the ankle. The fracture was a complete one, and the ends of the bones were overlapping to a slight extent. The bones having been placed in proper position, side splints of wood were applied to the limb, which was then secured to a firm pillow. For several days the patient was very restless, and the splints were constantly being disarranged, and the fractured bones displaced. I therefore, ten days after the injury, put up the limb in pasteboard and starch, so as completely to prevent any movement of the bones. This apparatus was removed at the end of ten weeks, when the fractured bones were found to be firmly united. The pressure of the splint had caused two superficial ulcerations, one over the outer ankle, and the other over the head of the fibula. Two weeks after this the sores were soundly healed, and the patient appeared none the worse of his accident.

# Part Second.

## REVIEWS.

*A System of Surgery, Theoretical and Practical, in Treatises by various Authors.* Edited by T. HOLMES, M.A. Cantab., Surgeon and Lecturer on Surgery at St George's Hospital, etc., etc. Second Edition, in five volumes, with Illustrations. Vol. II.—Local Injuries. Pp. 937. London: Longmans, Green, and Co.: 1870.

IN pursuance of the plan commenced in the first volume of the second edition, the papers are arranged with still more attention to order than in the first edition, and this volume contains Local Injuries. In size it only exceeds the former one by some 40 pages, but its contents vary considerably; for it begins with upwards of 100 pages on Burns and Scalds and the general pathology of Fractures and Dislocations, which were in the first volume of the first edition, and does not contain the excellent and very full treatise on Diseases and Injuries of the Eye, with which Mr Dixon filled the last quarter of the corresponding volume in the previous edition.

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[1] See Travers's Further Inquiry, p. 436; also Bushe, in Medical Times and Gazette for April 10, 1840.

All the other papers are carefully revised, and all are slightly increased in length.

Professor Longmore's excellent paper on Gunshot Wounds is considerably enlarged, specially on the subject of treatment of gunshot wounds of the lower extremity. The paper on Injuries of the Head is little altered either in size or arrangement. Mr Shaw's valuable paper on Injuries of the Spine has been carefully revised, indeed almost rewritten, and is considerably longer, chiefly from the addition of many new and important cases, most of which bear on the subject of railway injuries, and the subsequent trials for compensation.

While Mr Holmes Coote has made little change on his paper on Injuries of the Face, Mr Durham has rewritten and very greatly improved and enlarged the paper on Injuries of the Neck by the late Mr Henry Gray. He has given some tables of very great value. One at p. 488 gives a brief view of the result of analysis of 554 cases of foreign bodies in the air-passages. Among other principles to be learned from it, it chiefly illustrates the advantage of operation; for of 271 cases not operated on, 115 died; while of 283 operated on, only 70 died. His account of the operations on the air-passages, and especially of tracheotomy, is very valuable and complete. He most decidedly expresses his opinion that tracheotomy should be performed below the thyroid isthmus. The substance of his own careful and original paper on the method of performing the operation, which appeared in the "Practitioner" for September 1869, is given here.

After discussing the nature and treatment of cases of foreign body in the Pharynx and Œsophagus, a table is given of the cases in which the operation of œsophagotomy has been performed. His table in the first edition contained eight cases only; in this edition twenty-one are recorded, and of these nine, all successful, have been performed since 1861.

A similar table is given of the cases in which gastrotomy (or, as Sédillot would like us to call it, *gastro-stomie*) has been performed. While in the edition of 1861 only six cases were on reçord, Mr Durham has collected in the edition of 1870 no fewer than sixteen; of these nine were performed on account of obstruction, the result of malignant disease, and all died; and the remaining seven were performed for the removal of foreign bodies, such as knives and bars of lead; and all seven recovered, most of them very rapidly.

Mr Poland's paper on Injuries of the Chest has also undergone considerable revision and enlargement. The section on Wounds of the Heart is greatly enriched by very free quotation from Dr Fischer's paper on Wounds of the Heart and Pericardium, which appeared in Langenbeck's Archiv for 1868. This paper gives a minute analysis of 452 cases of wound of the heart, and is chiefly remarkable from the number of recoveries which it records—at least seventy-two having recovered, and in fifty-seven more the result having been uncertain.

Mr Pollock's account of Injuries of the Abdomen is little enlarged; but one important section, that on Artificial Anus, is rendered much more intelligible by the insertion of some woodcuts.

The remaining sections of the work on Injuries of the Pelvis and of the Upper and Lower Extremities are not much altered either in size or material. Mr Hulke has re-edited his colleague's paper on the Injuries of the Upper Extremity with his usual care and thoroughness.

These last three papers specially, but indeed all in the second volume, have been greatly increased in value by the insertion of numerous woodcuts (139 in number). These are less pretentious than the chromo-lithographs of the first volume, but better executed and much more practically useful.

On the whole, this volume of the re-issue is a great improvement on its predecessor, both in material and arrangement.

## *Our Domestic Fireplaces.* By FREDERICK EDWARDS. New Edition, Illustrated by numerous Engravings. Longmans, Green, and Co. London: 1870.

THE work before us, on the Construction of Domestic Fireplaces, though not strictly "medical," yet is so intimately connected with the preservation of health and prevention of disease, that we have pleasure in looking into it, and conveying to our readers a short account of its purpose and contents. We do so the more readily, considering that "open fireplaces" are now so much approved of, and occupy a most important place in the heating and ventilation of the wards of hospitals and other public buildings, as well as in our private abodes.

This treatise is more of a popular than a professional one; but we purpose to look upon it rather with the eye of a "*hygeist*" than of an ordinary reader, in order to cull from it those improvements and recommendations which the author suggests for comfort and economy in the use of fuel in domestic fireplaces; conceiving as we do the great influence of these on human health and happiness, by the supply of congenial heat and necessary ventilation.

In the first chapter of the work before us, the author gives a very full account of the origin and history of the use of fuel, and the invention and use of chimneys, vents, grates, and stoves, for domestic purposes. And it is both interesting and curious to be made acquainted with the progress of these, as affording very considerable information regarding the domestic history and progress of civilisation of the human race.

Some imperfect arrangements had been attained by the Greeks, Romans, and Chinese, for the domestic use of fuel; but these were probably unknown in this country till introduced by the Romans. In far back primitive times, when fuel was first used within the small tents and cot-houses of our ancestors, these possessed only an opening in the centre of the roof for the escape of smoke and foul air, an arrangement which must have proved both unpleasant and unhealthy. But this was the only method known and used, even till a very recent date, by the nobles of the land and in the castles of the opulent. Flues and chimneys were afterwards constructed, in connexion with the side walls, when better houses were built. Until the fourteenth century, according to our author, fires were only made use of in the open air for cooking and other purposes. They were afterwards employed inside for warmth, as well as for other domestic uses, when chimneys had been invented.

In the seventeenth century great improvements were made in the construction of chimneys, and in their extension to the different rooms in the upper floors of the houses. But still they seem to have been very imperfect; for they required the additions of a "fire-cloth" in front, and "bellows," to cause the combustion of the wood employed as fuel. These expedients were evidently employed to increase the draught in the vents by contracting the chimneys at the fireplace. In these times also, the "folding screen," to prevent cold draughts of air in rooms, and the use of "warming pans" for beds, were found to be necessary adjuncts for comfort; but they have now almost entirely disappeared by the use of coal and the superior arrangements within all modern houses, which have followed the increase of population, industry, intelligence, and wealth in our land.

In the year 1239, a charter was granted to the inhabitants of Newcastle to dig for coal. It then became imported into London for the use of brewers, dyers, smiths, and others; but it was so obnoxious to the public by making the air foul, that Parliament, in 1306, petitioned the king to prohibit the use of it within the city. And it appears that in the time of Edward I., a man was tried and executed for the crime of burning sea-borne coal in London,—a severity caused by the fears then entertained of coal fires being dangerous to the community by setting fire to their houses.

After a considerable time, by greater familiarity with the use of coal, and experience in its uses for heating and cooking, as well as in trade, it became gradually introduced to the domestic fireplace.

But we must forbear extending our historical remarks; for, however interesting, to enter more fully into these would be superfluous and out of place. We shall now confine our review, therefore, to those points which bear upon *health* solely. Keeping this in view, our attention will be directed to ascertain from the work before us, (1.) the best arrangements in our domestic fireplaces for imparting heat, and (2.) as a means of ventilation.

It would be only a waste of time and space to follow the author's minute history and details of the various inventions, with alterations and improvements upon them, which have been made for the twofold purpose above mentioned. We shall, therefore, endeavour to extract from his treatise what has been found most effectual and most approved of in regard to it.

It was not until the end of last century that real and permanent improvements were made in the construction of domestic fireplaces, all the previous—and many of them very ingenious—inventions having failed to effect the required conditions found to be necessary. None of them having been successful in burning fuel in domestic apartments without smoke and other evils, they were, one after another, soon abandoned and forgotten. A very ingenious individual, Count Rumford, had then come to this country, and applied himself to improvements and economy in the application of fuel to cookery,—preventing smoke, and, in other respects, securing the greatest amount of benefit from it. He seems to have discovered the real cause of the previous failures of others, viz., the excessive width of the chimneys; and by suggesting the contraction of grates and fireplaces with bricks, he not only attained the success desired, but was actually the inventor of the greatest improvement which has been made either before or since his time, by his combination of iron with bricks in the construction of the grates he recommended.

The grates upon this principle, now very generally in use on account of their suitable properties, consisting of a front of iron and fire-bricks within, and known in England under the name of "sham grates," and in Scotland as "Kinnaird grates," by commanding a good draught in the vent, are the most effectual for burning the fuel, curing smoke, and imparting heat. Hence, modifications of these, as to size, construction, and having more or less ornament, may be observed in the best grates manufactured since Count Rumford's time. The admirable engravings in the work before us, showing the Count's inventions, are an exact delineation of the "sham" and "Kinnaird" grates above alluded to. We need scarcely add, as it is almost universally known, that these grates have contributed in a very great and remarkable degree to the comfort and healthfulness of our domestic houses. When we contrast the total want of domestic fires, or even the early and rude use of fuel, within the habitations of our ancestors, with the perfection and luxury now so universal in this respect, we cannot help remarking the vast improvement which must have taken place in the health of the community, and, consequently, on longevity. Without inside fires in winter, or with fires and open roofs for the escape of smoke, in northern climates, both health and life must have been very precarious, and difficult to be retained; and even the strongest constitutions must have been placed in very unfavourable circumstances for recovery from disease.

Of late, new grates have been brought into notice, constructed on the model of the old-fashioned "dog grates," and have superseded others in many public rooms of our best houses. These require a large fireplace, as they are wide and open; and are richly ornamented with figured tiles inside and in the hearth. These give an elegant appearance to the fire; but they are found to give out less heat, and more apt to cause smoke in the room, than others. These objections, however, have in some measure been removed by the addition of fire-bricks at the back, and the contraction of the chimney above the fire for the better escape of the smoke.

While the slow combustion of the fuel has always been considered to be an object of importance, both for economy and the retention of heat, another property, which requires quite an opposite condition, has also been anxiously sought after in the construction of grates—namely, that of the fire consuming its own smoke. The inventions and modifications employed for this purpose were all in effect calculated to consume the fuel more rapidly and effectually, by the more free admission of air and stronger draught,—by adding coal in small quantities at once, and placed loosely (not packed) upon a strong fire. These inventions were generally found to be too complicated; and while they consumed smoke they drove off the heat; and it seems now established that the common and more simple grates of Count Rumford, above described, can be made to answer all necessary requirements, and are therefore found to be of the best form for use. In consuming the smoke, which pollutes the air in towns, another evil is created, by the conversion of it into carbonic acid and other noxious gases, which, by their greater weight than that of air, fall to the ground. We therefore entertain the notion that there is a *safety* in the blackness of smoke and in the strong odour of coal gas, which is wanting in carbonic acid and other colourless and deleterious gases. So that no great object, in our estimation, is to be gained by what has been termed "the combustion of smoke," as we consider it to consist only of the conversion of one noxious vapour into another equally so, and more dangerous by being invisible and inodorous.

We will conclude our remarks on this part of our subject by allusion to the Report of the Commissioners appointed by Parliament at the request of the Board of Health several years ago, to inquire into the whole subject of the Warming and Ventilation of Buildings. In the Report which they issued, they recommended what were called "smokeless grates" in preference to others,—polished surfaces to grates for reflecting heat,—ash-pans in ash-pits for cleanliness,—apertures at the back of grates for smoke,—fire-bricks for their interior,—and that the fireplace should have a good frontage, seen from the greatest number of points in the room. They also added that the construction of the fire-grate should be studied for warming only, and not for ventilation. In this, however, they must have had in view, that an open fireplace must always be a powerful ventilator when used for warmth, even with the greatest economy. They also advised that the warmth of rooms should be preserved by double windows; and that a supply of fresh air should be introduced in proximity to the fire. It does not appear that these recommendations, however valuable for utility and economy, were treated with much regard, or acted on generally, by the public.

In contrast with the close stoves, thick walls, double windows, and carefully-fitted doors, employed to exclude air and to prevent loss of heat in the winter of the less genial climates of other countries than this, our easily-regulated, cheerful, open fireplaces will not soon be superseded; they may be improved, but are not likely to be set aside. Indeed, from the construction of our houses, and the long-cherished social enjoyment to which our blazing fires contribute in relieving the long, dreary, and cold winter nights around the domestic hearth, the open fireplace, with its glowing heat, cheerful light, and benignant influence, may now be considered as an established national institution not to be exchanged for any other system.

The concluding chapter of the work before us treats of close stoves, hot air, and hot water modes of obtaining and circulating heat in domestic and other buildings. These having been extensively used in this and other countries, the merits and demerits of each of them have been fairly tried and tested. The result appears to be that, however effectual for heating, they are much inferior as to salubrity to open fires, more especially in small rooms, from the want of proper ventilation; and that for heating large halls, churches, and other public buildings, the circulation of hot water, by means of pipes ramified through them, forms the most efficient and best mode of obtaining the desired object.

We must now conclude with observing, that much praise is due to the author of the work of which we have given an account, for the valuable information he has collected, illustrated so fully by engravings, and made public by this treatise, on a subject of such universal interest. It presents to us a complete view of the matter of which it treats, showing what has been done, the success attained, the defects existing, and the more perfect conditions yet to be acquired; thereby pointing out fields to be cultivated by public exigency and individual enterprise for their benefit, by contributing to the health and happiness of the future generations of our race.

*Forms of Animal Life: Being Outlines of Zoological Classification based upon Anatomical Investigation, and Illustrated by Descriptions of Specimens and of Figures.* By GEORGE ROLLESTON, M.D., F.R.S., Linacre Professor of Anatomy in the University of Oxford. London: Macmillan and Co.: 1870. Pp. 436.

*Introductory Lecture to the Course of Comparative Anatomy, delivered at the Royal College of Surgeons of England.* By WILLIAM HENRY FLOWER, F.R.S., F.R.C.S., Hunterian Professor. London: Churchill and Sons. Pp. 43.

THE work of Professor Rolleston, at present before us, is one of the most important contributions to zoological literature which has been produced of late years. Written by one of the most conscientious and laborious of living workers in the field of biological science, enriched by the results of the writer's unrestrained access to one of the finest scientific libraries in the world, and printed at the Clarendon Press, it would, indeed, have been a matter for wonder if we had not in this a work of permanent weight and value. It may be doubted, in fact, if any work of modern times, which embraces the entire range of zoology proper, and is not a mere monograph, will be found more useful than this to teachers and advanced students of the science. We say advisedly to *teachers*, for we do not believe that the work is one which could be placed with any advantage in the hands of ordinary students. The author, in his preface, states that the work was written "with a view chiefly to the needs of university students of comparative anatomy;" and it is possible that it may be found to fulfil its author's expectations so far as Oxford is concerned, where the students of this branch are few in number, are under the direct supervision of their teacher, and have a more than ordinarily superior general education. We have no hesitation, however, in asserting that the work is one which would be next to useless to ordinary students just commencing the study of zoology, and not desirous of acquiring more than a general outline of the science. Granted a far from inconsiderable preliminary acquaintance with scientific method and terminology, with natural history itself, and with some of the collateral sciences, and the reader would find himself presented with an orderly and systematically-arranged mass of facts and generalizations which would occupy his mind and task his powers of digestion for many a day. Wanting this preliminary knowledge, of itself far from general or easy of attainment, the student would find himself hopelessly involved in a labyrinth of technical terms, expressive of unknown and bewildering anatomical facts, unexplained by any glossary, and often rendered still more obscure by their connexion with important generalizations. We repeat, however, that for *teachers* of zoology—and the number of these is a yearly increasing one—we know of no single work of greater absolute value, or presenting its materials in a more available form.

The work consists of three parts. "The first is an introduction, giving a classification of the animal kingdom, with a zootomical account of its various sub-kingdoms and their subordinate divisions and classes; the second consists of descriptions of certain readily procurable specimens which illustrate, in the concrete, a very large number of the systematic descriptions contained in the introduction; and the third contains descriptions of figures supplementary to the descriptions of specimens, and intended to aid them in furnishing that groundwork of particular facts, without which it is impossible to obtain any real knowledge or permanent hold of general principles."

Leaving the first part of the introduction, which calls for no particular notice, we have in the remainder of this section of the work a series of definitions of each of the sub-kingdoms, and of all their primary divisions and classes. In some cases, definitions of the orders are added, and it would have been a decided advantage to the student if this had been generally carried out. The classification which has been adopted appears to be always, or almost always, the one proposed by Huxley, now so well known to all students of zoology. There are instances, however, in which this classification might well have been departed from, if only for the sake of convenience. The subdivision, for example, of the class Aves has long been a stumbling-block to systematists from the extreme compactness of the group, due to their peculiar mode of life; and it is questionable if the classification adopted here by Huxley is one which should be followed. Huxley, namely, divides the whole class of the Birds into no more than three orders—the *Carinatæ*, the *Ratitæ*, and the *Saururæ*. The first of these includes all birds with a well-developed sternal crest and considerable power of flight; the second includes those birds in which the sternal keel is wanting and the wings are rudimentary (*e.g.*, the ostrich and emeu); and the third comprises only the very aberrant extinct bird, the *Archæopteryx macrura*. It cannot be denied that in many respects this classification is a very natural one, and is a great improvement upon the older systems. At the same time, its inequality is such as to render it almost valueless to the student; since under *Saururæ* we have only one bird, under *Ratitæ* no more than four genera, and under *Carinatæ* all the remaining members of this exceptionally large and varied class. Upon the whole, however, and in most respects, Huxley's classification is unequivocally preferable to any that has hitherto been proposed, and Professor Rolleston is therefore amply justified in its adoption; though it is to be greatly regretted, for the sake of students, that there is hardly a single division of the vertebrate sub-kingdom to which different names are not applied by such distinguished authorities as Professors Owen and Huxley. This is the more to be deplored, as in not a few instances the names given by Owen appear to have the priority, and should therefore be unhesitatingly adopted.

In connexion with the definition of the *Protozoa*, we have an interesting digression on the means by which animal and vegetable organisms may be distinguished from one another. The author properly disapproves, in the meanwhile at any rate, of Haeckel's proposal to form a "Regnum Protisticum," or third kingdom of life, intermediate between the animal and vegetable kingdoms. He concludes that in the case of "microscopic organisms, about the position of which it is possible to raise a doubt, the most unambiguous criterion is that which the formation of an external envelope of cellulose, when present, furnishes. When this means of deciding is absent, the exhalation of oxygen, and the power of supporting life upon inorganic matters, are perhaps the two points to which we should next look, though both of them would fail us in the case of the Fungi. The presence of chlorophyll in very great abundance in the parenchyma of an organism points, though less certainly than any of the three characters already specified, to the vegetable character of an organism. . . . An organism which should be seen to envelop alimentary substances within its own parenchyma would be, in almost any case, rightly considered an animal; but this test would fail with the entoparasitic forms of either kingdom which live by the absorption of the soluble nutriment in which they live immersed at all points of their exterior; and would further be held by most naturalists to prejudge unfairly the allocation of organisms feeding by means of suctorial pseudopodia, in the face of the preponderating evidence in favour of their animality which the totality of their history offers. It would fail, also, in the cases of the spermatozoa, which may be called the male 'gonidia' of most animals, and the male zoöids, the 'complemental males,' of a few animals such as the *Cirripedia*. . . . It may be anticipated, that in the few cases in which it may at present be difficult to decide with perfect certainty as to the animal or vegetable character of an organism, an increase of our knowledge, if not of its very simple structure, yet of its development, and if not of either its development or its structure, yet of the development and structure of forms which by gradual transitions connect it with undoubted animal or undoubted vegetable forms, is likely at some time to enable us to place it in one or other of these two kingdoms of life. But it must be said that there are organisms which at one period of their life exhibit an aggregate of phaenomena such as to justify us in speaking of them as animals, whilst at another they appear to be as distinctly vegetable. A monad may at one period be possessed not only of a nucleus and contractile vacuole, but of a cilium, by the aid of which it swims about; at another, it may have lost its cilium, and effect locomotion by the protrusion of pseudopodia, like an Amœba; whilst in a third, it may surround itself with an envelope of cellulose. If it should prove to be true that organisms as high in the scale as *Amœbina* and *Actinophryna* can have their development traced back to the specialization of protoplasm within vegetable cells, it would appear to be necessary to adopt a phraseology which should speak of such creatures as being at one time plants, and at another animals."

The second part of the work is occupied with detailed descriptions of fifty preparations, taken indiscriminately from most of the classes of the animal kingdom. The value of these is rendered still greater by short accounts of the methods employed in the preparation of most of the specimens described, so that it would be possible for the student, with a little trouble, to reproduce the more important ones for himself. The last part of the work is taken up with a full description of twelve plates, the figures of which are either drawn from actual dissections, or, where the organisms are minute, are of a diagrammatic nature. A noticeable feature is, that the descriptions both of the preparations and the plates are accompanied by a valuable and well-chosen series of bibliographical references.

In conclusion, we have only to say, that whilst the subject-matter exhibits profound research, whilst the plates are clear and well executed, and whilst the printing, as a matter of course, is above the average, the style of the writer is decidedly laboured and involved, and is often far from exhibiting the lucidity which is desirable in a work of this nature. There are also a few points of detail which are not free from objection, some of these being apparently intentional, whilst others are probably due to oversight. Thus, it is very unusual, to say the least of it, to find the *specific* names always and uniformly prefixed by a capital letter, whether derived from a proper name or not. Myri*o*poda, again, is an unwonted form for Myri*a*poda; and the oceanic Actinozoa are sometimes spoken of as Ctenophor*æ* and sometimes as Ctenophor*a*. Lastly, it savours somewhat of pedantry to employ the term *phaenomena* in place of the generally accepted and current form of the word.

Professor Flower's Introductory Lecture requires little notice. Though intended to be an introduction to a course of lectures on Comparative Anatomy, it is really nothing more than an examination of the value of the Darwinian hypothesis, especially in its bearings on Classification. The conclusions arrived at by the author may be summed up in a passage quoted by himself from Professor Asa Gray:—

"Those, if any there be, who regard the derivative hypothesis as proved, must have loose notions as to what proof is. Those who imagine it can be easily refuted and cast aside must, we think, have imperfect or very prejudiced conceptions of the facts con[illegible]d, and of the questions at issue."

### *A Treatise on Asiatic Cholera.* By C. MACNAMARA, Surgeon to the Calcutta Ophthalmic Hospital. London: 1870.

THIS is another book on cholera from India, full 8vo, 560 pages. It is dedicated to the Duke of Argyll, and contains a well-bestowed compliment on Lord Mayo,—a good old practice, almost forgotten by the bookmakers of this century. We dó not know if either of these two great functionaries read the Medical Journal, but if they do, we have the honour to inform them that this is a good book, the result of much work, thought, and research. Though it may seem long for a monograph, it goes over a great deal of ground, and is full of interesting matter, communicated in a clear, flowing, and correct style. We say this without professing to agree with all the views held in the work, a great part of which is devoted to a history of the diffusion of cholera over the world. The author is a zealous advocate of the view that the disease is spread by contagion; hence his statement of the history of different epidemics will be disputed at every stage by those who set out from a different standpoint. Let any one, for example, compare Mr Macnamara's account of the passage of cholera to America in 1848 (p. 118) with that of Dr Wood (Practice of Medicine, vol. i. pp. 681 and 692; Philadelphia, 1855), and he will see how the same events may receive entirely different explanations. An attentive examination of what are called by compliment "the facts" about the introduction of cholera here and there has convinced us that often they are no facts at all, but mere wreaths of theory or guess-work taking the form of an assertion. Where two or three observers meet, they almost always contradict one another on some important point.

According to Mr Macnamara (p. 221), cholera was introduced into Guadeloupe in 1865 by a ship from Marseilles, one of whose passengers died from the disease during the voyage; and the first victim was the woman who washed his clothes. Turning to Dr D. B. Smith (Report on Pilgrimage to Juggernauth, etc., p. 10), the ship, apparently the same one, had not shown, "as we are assured, a trace of cholera on board." Of course they quote different authorities; and one would like to know which is right.

The leading idea of Mr Macnamara is, that cholera is propagated, in the great majority of cases, by drinking water becoming mixed with the drainage of choleraic evacuations. This view, perhaps suggested by the mobs of Paris and Madrid, who insisted, when the cholera first appeared, that the wells had been poisoned, was put into a scientific form and advocated by Dr Snow. One could not object to the possibility of such an occurrence, which we ourselves imagined to be strongly confirmed by experiments upon the lower animals, to which Mr Macnamara now deals a cruel blow. "I have repeatedly," he writes, "endeavoured to produce cholera in dogs, cats, and other animals, by injecting the material of cholera dejected into their stomachs; but I have invariably failed to produce any effect." These experiments were repeated in a variety of ways, but always failed. "I am aware," he goes on, p. 416, "that mice are said to be subject to cholera. Dr Burdon Sanderson would seem to have proved this very conclusively; but I would caution those who may wish to substantiate his observations, in the first place to keep white mice for some time before beginning to try experiments on them so as to study their habits; and then, in making the experiments, to take an equal number of these little creatures, feed one set on paper prepared as Dr Sanderson describes, and let the other set feed on paper which has been dipped in decomposing organic matter—in fact, keep two sets of white mice under precisely the same circumstances with regard to food, water, and so on, with the exception of one set having cholera dejecta mixed with their food, and the other decomposing organic matter other than cholera fomes. Experimenting under these conditions, I have failed to convince myself that white mice are capable of being affected by the alvine dejecta of cholera patients."

Mr Macnamara lays much stress on a case where it was certain that water contaminated by fresh choleraic dejecta from a patient was swallowed by nineteen people, and that five of these took the cholera within the third day, the remaining fourteen escaping from all uneasiness. He assures us that there was no cholera prevalent in the place, that it had not visited the locality for several years, and has not, he believed, appeared since. But if there were no cholera in the place, where did the fresh choleraic dejecta come from? Was the cause that made one man ill of cholera not good to produce it in five more? Probably it would not help us much if we were told where this occurrence took place. We can only gather by the context that it must have been in India, and in the year 1861; but the author does not say that this was his own observation or somebody else's.

When Mr Macnamara appeals to the history of the Hurdwar epidemic in 1867, as a further illustration of the spread of cholera by infected drinking-water, he leaves himself open to the suspicion that this instance is only appealed to for want of a better; and that, so far from it affording proof of his hypothesis, it is only by suppressing some important facts that the occurrence of such a universal contamination of a river like the Ganges is made to appear anything more than possible. For some account of this occurrence the reader may turn to a review of Dr Cunningham's Report in the July number of this Journal; and the reader of Macnamara's work ought to bear in mind that the camp at Hurdwar was under strict medical supervision, and that for the first time; that there were no cases of cholera at all amongst the pilgrims, only one sporadic case in a grasscutter attached to the 14th Bengal Cavalry at Kunkhul, a place on the Ganges Canal considerably below the pilgrims' bathing-ghat on the river itself. The health of the pilgrims was wonderfully good, bowel complaints being rare. Mr Macnamara believes that the pilgrims who had been drenched all night by the rain of the 11th of April were infected at the bathing-ghat on the 12th, when they went in a body to bathe themselves in the holy river, and when each pilgrim drank a little of the water as a religious ceremony. The contagious matter was furnished, we are to believe, by the clothes of those pilgrims who had come from infected districts; he does not say what districts. Eight cases were reported on the 13th, on which day the vast multitude had dispersed. Our author does not tell us how the pilgrims bore the cholera over the North-West, through the Punjab, and into Cabul and Cashmere. Are we to suppose that within a few weeks the pilgrims contaminated the drinking-water of all these countries? Contamination of drinking-water may occasionally take place, and we are not prepared to gainsay that this may sometimes produce cholera, but how could it be accomplished without any design over such a wide area, with such celerity and certainty, and that during the driest months in the year? Or where the one hypothesis fails, are we to resort to another?

There are some other objections founded on the history of previous fairs; but enough has been said. Dr Cunningham strives to weaken the improbability of a copious and rapid river like the Ganges being contaminated by what must have been a very minute amount of "contagium," by telling us, that in the late epidemic, "the discharges of two patients are supposed to have contaminated the river Lea, and so to have spread the epidemic over East London." But, in truth, we cannot suffer one disputed fact to be used to mend another. That we can prevent cholera simply by guarding our drinking-water, will be good news to cities like Edinburgh and Glasgow, which get their water mainly from mountain lakes, easily guarded from specific contamination. Professor Hallier, of Jena, has advanced that cholera is produced by *micrococci*, described as particles of plasma without any cell-walls, but which are capable of further development. The Indian Government has commissioned two assistant surgeons, whose capacity for conducting an independent scientific investigation was assumed from their being first on the list at the examinations for the British and Indian Medical Services, to commence an extended inquiry into the origin of cholera. These gentlemen were sent to Germany to make themselves acquainted with the views of Hallier, Pettenkofer, and De Barry, after which they went to Calcutta. As far as their investigations in India go, no peculiar species of fungi have been found in choleraic evacuations, nor have those found been recognised as belonging to the cholera series of Hallier.[1] The learned Professor's views about the development of the *micrococci* have also been questioned by some observers in cryptogamic botany. The author prophesies that cholera will now appear much more frequently in Europe, since, by the completion of the Suez Canal, the crews of ships from Indian ports will reach Europe, when formerly they were detained at Suez.

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[1] See Fifth Annual Report of the Sanitary Commission with the Government of India; Calcutta, 1869, p. 26.

The chapters on the morbid appearances and symptoms of cholera are amongst the best in the book. He finds the characteristic feature of cholera in the changes in the epithelial cells of the intestines, which take place during the life of the patient. Molecular matter appearing under a high power as small specks in the epithelial cells is, he conceives, "formed in and at the expense of the epithelial cells, blood-globules, gland-cells, or, in fact, any organic matter brought within its influence."

"It seems to me," he goes on, "that this molecular matter, if introduced into the intestinal canal of men, is capable of setting up during life, in the cells lining the intestines, an action similar to that by which it was itself produced; but whether this conversion is due to chemical affinity, or is brought about by the introduction and growth of a cholera germ, or 'cholera zymes,' as Dr W. Farr calls it, I am at present unable to determine. But I believe that this rapid molecular formation in the epithelial cells of the intestinal canal is peculiar to the disease we call Asiatic cholera, and in no other malady is anything similar witnessed, if we take into account the rapid course which cholera runs, the epithelium being invaded by this matter in the space of a few hours." Mr Macnamara believes that the destruction of the epithelium of the passages of the nose and mouth takes place during life, but that the destruction of the epithelium of the kidneys and lungs is, in great part, a post-mortem appearance. According to Thudicum, the cerebral vessels are sometimes obstructed by the debris of epithelium and blood-corpuscles from the dura mater and arachnoid.

He regards sulphate of iron as the best disinfectant for choleraic evacuations, and has no faith in chlorine.

Medical men in India expect much from preventive measures; but experience has taught them to hope little from treatment. The treatment which the author recommends seems in some degree to be influenced by his theory. He gives thirty drops of laudanum to arrest the first stage of the disease. This may be repeated in half an hour if the purging be not stopped. The patient is to be kept reclining, and if thirsty to get iced water, acidulated with dilute sulphuric or with citric acid. The author believes that it is a very grave error to neglect arresting diarrhœa at once during the prevalence of cholera, and thinks that opium should always be within reach to be used for this purpose. It may be objected to this, that the patient by drugging himself may be induced not to attempt consulting the doctor or apothecary, which, in a regiment in India, may generally be done at once. The nature of the diarrhœa could thus be ascertained, and further inquiries made; for if it be choleraic, the patient has, if we adopt Mr Macnamara's view, been exposed to a contagious poison, probably in his drinking-water.

Possibly he or his fellows may be exposed to the same cause again. Moreover, in some kinds of diarrhœa, opium is not a proper remedy. The destruction of the cholera virus, or at least the arrestment of its progress, by a grain or two of opium, or even some simple astringent, is a circumstance deserving of our attention. Observe, we do not deny it.

He continues opium in the beginning of the second stage, and then gives sulphuric, acetic, and other acids, with the intention of making the stools acid, so as to redden litmus, when he stops their administration. He gives cold water and ice to relieve thirst, and uses frictions against the cramps. He protests against alcoholic stimulants in the second stage.

Where the patient is very restless, he gives inhalations of chloroform, which he has done without any apparent injury in the very last stage of collapse, with the temperature of the body as low as 92°, and the respiration at 50. During collapse, Mr Macnamara expects little from remedies, and cautions us not to allow our eagerness to help the patient to urge us to push medicines down his throat, which can only do him harm. He forbids opium and stimulants.

If the urine is suppressed during reaction, he uses tincture of cantharides. He recommends a mild farinaceous diet till recovery has well set in.

Altogether this is a book which does the author much credit. His main aim is to call attention to the preventive measures by which he believes the ravages of the "Indian pestilence" might be restrained; but the only way we can do justice to his argument is to advise those interested in the matter to read his book.

## *Syphilis Popularly Considered.* By JAMES GEORGE BEANEY, F.R.C.S., etc. Pp. 297. Melbourne: George Robertson: 1869.

WE have generally understood "Popular Syphilis" to be the special sphere of quacks and medical advertisers, and after reading our author's production, we do not feel inclined to alter our opinion; for though there is a considerable amount of useful information in the book (if it had been bestowed in the proper direction), there is so much abuse of professional brethren's opinions and treatment, and so many astounding and horrifying cases introduced, almost all of which, wonderfully enough, end in a permanent cure, that we cannot divest ourselves of the idea, that what we have before us is neither more nor less than an advertisement.

The book is totally unfit for the eyes of non-professional readers. There is one part which might be readily enough (perhaps too readily) understood and believed by the community at large—namely, the chapter on the communication of syphilis, wherein our author ventures to use very strong language with regard to vaccination, describing the present system as "bungling" and "blundering," and a means of sowing syphilis broadcast. In the present state of public opinion in some quarters, this tone of writing is, we consider, most uncalled for, and most injurious to all concerned.

There are some points in the pathology and therapeutics of this work which we should be inclined to criticise were the work intended for the notice of the medical profession. The accuracy of the following statements we consider at least doubtful—that *four* hard chancres have been found on the same person—that chancres have been found in the brain substance—that phthisis may be communicated by vaccination (query *tubercle* by *inoculation*)—that the greater proportion of incurable internal diseases is due to syphilis—and that the greater number of general practitioners are quite incapable of recognising or treating syphilis.

We think we have said enough to convince our readers that the book is not worthy their perusal.

Our author promises to publish soon a work on syphilis for the benefit of the medical profession. We shall be delighted to see it and give it a fair consideration. As we anticipate that all his opinions will be reproduced in the new work, we refrain from saying anything more of the present publication.

*As regards Protoplasm in relation to Professor Huxley's Essay on the Physical Basis of Life.* By JOHN HUTCHISON STIRLING, F.R.C.S., LL.D. Edinburgh: William Blackwood and Sons.

*Force and Matter in relation to Organization: An Introductory Lecture to a Course on Physiology.* By ARTHUR GAMGEE, M.D., F.R.S.E., etc. Edinburgh: Maclachlan and Stewart.

A FEW years ago there came amongst us as pastor to one of our so-called Congregational churches one who has since then passed away—as he, perhaps, would choose it to be thus vaguely put. Singularly earnest in all he thought or did, his life was spent in teaching, both by precept and example, that the spread of truth among mankind was all that is worth living for, and that even death, in such a cause, is not to be feared; though beyond death nothing but truth—be that what it may—may seem to be certainly attainable. Whatever we may think of his peculiar views, or however we may lament their one-sided tendencies, none can deny that the Rev. James Cranbrook lived and died a martyr in the cause of truth, and that it would be well for mankind if those who think themselves more orthodox possessed a tithe of his earnestness and self-sacricing spirit. At Mr Cranbrook's suggestion Professor Huxley delivered, one Sunday evening, his admirable lecture on Protoplasm as the Physical Basis of Life; to which Dr Stirling has published the rejoinder, which forms the subject of our present notice. Essentially a popular lecture, and delivered before an unscientific audience, Professor Huxley did not make a parade of scientific authorities, nor did he enter into all the scientific relations of his subject, which could not have been appreciated and would not have been understood by his audience, but contented himself with stating, in a sketchy and popular manner, that the physical basis of life is not a cell or a nucleus, but the cell-contents or protoplasm; while life itself, and all its phenomena, is not an entity, nor, so far as we know, a special force—though it may be such—but merely a conventional expression for the phenomena which result when protoplasm is brought into contact with extraneous matter. Life, therefore, is a property of protoplasm—that is, of organized matter—the properties of a substance being, as Thomas Brown put it long ago, only the substance itself considered in relation to the various changes which take place when it is placed in peculiar circumstances. All this is from one point of view quite incontrovertible; the chief question at issue, therefore, is the source of this protoplasm; and that we believe to be, as yet at all events, insoluble by any means at our disposal. We do not believe that Professor Huxley for one instant desired his audience to believe that the so-called protoplasm in the cell of the nettle was actually identical with that in human flesh, any more than he believes that the cell-contents in the kidney are identical with those in the brain, as Dr Stirling seems to wish us to believe he does. No one knows better than Dr Stirling that though brain-cells only generate brain-cells, and bone-cells only bone-cells, etc., all alike have arisen from one cell—the germ-cell—whose contents, protoplasm, are capable of being differentiated into all the various cells required in the more or less complex being to which the germ-cell was destined to give origin.

No one knows better than Dr Stirling that Professor Huxley no more believes that a hen's egg can produce an infant man, or a protococcus germ an oak, than he himself does; and that all that Huxley popularly put forward was his idea that life, in its beginning, is inseparably connected with a more or less definite protein compound identical in all animals and in all plants, which differentiates into man on the one hand, and into the higher vegetable form on the other. For all argumentative purposes, it is of no consequence whether we regard the nucleus, the cell, or the cell-contents—the protoplasm—as the organic unity, provided we acknowledge the chemical or physical identity of the basis from which all life springs; and this we believe to be undeniable, at least so far as the contrary cannot be proved, any more than it can be shown that the source of living action differs in anything but degree in the two kingdoms of nature. The ciliated germ of a vorticella and that of an alga exhibit movements so identical, and depending on such similar causes, that it is a mere begging of the question to say that the one depends on an automatic, and the other on a voluntary principle—whatever these words may be supposed to mean. It is quite true, that in their higher developments the two kingdoms differ essentially; the highest development of animal protoplasm thinks and expresses thoughts even sublimer than the "Iliad" of Homer, the highest development of vegetable protoplasm can only whisper vague nothings when swayed by the summer breeze. Both are influenced solely by external nature; the one, however, is so differentiated as to be able to express its thoughts intelligibly to other beings of its class; the other, whatever it may think, can only unintelligibly express its thoughts by vague physical movements and sounds.

The mere wincing of an animal under the knife so little requires the presence of conscious life, as to take place after the occurrence of somatic death; its cries, usually regarded as so eloquent an expression of the consciousness of pain, are vociferously poured forth when we know all such consciousness to be abolished, as after the removal of the cerebral lobes in animals, or, in man, after their complete stupefaction by the chloroform narcosis. All these phenomena therefore resolve themselves into the unconscious perception of an irritation, of which the animal can only give a cognizant expression when it has the free use of certain organs developed for that end. The ability to express the cognizance of an irritation otherwise than by automatic acts, therefore, only proves the existence in a perfect state of certain organs developed for that end, and does not warrant us to conclude that the nature of the life, living force, or psyche, differs in such organisms in any respect from that existing in those other organisms which can only reveal their sensations by those automatic acts. The phenomena, therefore, exhibited by plants which enabled Linnæus to construct his floral horologe, and many others of a similar nature, prove that in plants there is the same capacity for perceiving and acting upon an external stimulus as in animals; and there is no evidence to show that the vital force differs in any respect in the two kingdoms. And the same may be affirmed, we think, of the protoplasm or physical basis, which, though differing essentially in the brain-cells of man and the leaf-cells of the oak, has not been proved to differ, and probably does not differ, in the germ-cells of either, or in those lower organisms which it is difficult to say whether they belong to the animal or vegetable kingdom. The great question at issue between Mr Huxley and Dr Stirling is essentially the same as that so long debated upon between M. Pouchet and M. Pasteur, which is this: Are physical materials and correlated physical forces the sole efficient agents in the production of organic as of inorganic nature? or are all organic bodies the mere descendants of originally created similar organisms, each animated by a similar but not identical vital force, essentially different from, and not to be correlated with, any of the known physical forces? Such at least is our view of the question. Atheism we believe to be equally apart from the mind of each inquirer, and we regard Huxley as believing that the

Great First Cause is continually working by agents always present, while Stirling looks upon creation as long since ended, and regards reproduction as the sole law upon which the existence of organic nature now depends. Rightly regarded, neither view is inconsistent with man's highest hopes and aspirations; neither view is as yet capable of incontrovertible proof. But while we acknowledge that scientific, like political or any other form of truth, can only be advanced by slow degrees, and by the gradual modification of opposing views, and while we therefore also acknowledge the utility of the drag exerted by these opposing views, we cannot help saying that we think Dr Stirling has been somewhat unjust to Mr Huxley, in supposing or seeming to suppose him ignorant of statements and views with which we all know him to be fully as well acquainted as Dr Stirling, but which the popular character of his essay did not permit him to allude to, much less to controvert. Dr Stirling has even been unjust to himself in condescending to argue upon points which are incapable of incontrovertible proof. We sympathize with much that he has said, but we feel that, as he has said it, it hangs loosely together, and is not incapable of being similarly controverted. The question seems to be one more of probability than of scientific truth, and as all probability is against the Professor's views, it seems to us that it would have been better not to answer the Professor, except by a reiteration of the root-truths of our science, and a challenge to him to prove scientifically that life is ever found apart from germ-cells, or that germ-cells are ever developed except by reproduction. Apart from this, the similarity of the physical basis in all organized bodies no more proves its identity than the similarity of the vital force proves its identity in each germ; but if the identity in neither case can be proved, as we believe it cannot, then the argument derived from the existence of an apparently definite physical basis, as the substratum of life, is deprived of all the force which Huxley and his followers would fain assign to it.

Dr Gamgee's interesting lecture is a further contribution on the same question, and both it and Dr Stirling's pamphlet deserve to be carefully studied by those who wish to be well informed upon the points at issue.

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*Braithwaite's Retrospect of Medicine.* Vol. LX. London: Simpkin, Marshall, and Co.

*The Half-Yearly Abstract of the Medical Sciences.* Vol. L. London: John Churchill and Sons.

*Dobell's Reports on the Progress of Medicine.* London: Longmans, Green, Reader, and Dyer: 1870.

THESE volumes all aim at supplying the busy practitioner with a digest of all that has been done in medical science in the mos' recent times. The two first are old friends, which still maintain their usefulness and distinguishing peculiarities: the first giving more copious abstracts chiefly of the principal papers in British periodicals; the second taking a wider survey, but giving shorter notices. Dr Dobell's Report is more ambitious, and more cosmopolitan in its character: all three, however, though beyond the pale of criticism, are deserving of a place in the libraries of those who wish to be able to ascertain at a moment's notice the most recent advances made in the treatment of any given disease.

# Part Third.

## MEETINGS OF SOCIETIES.

### MEDICO-CHIRURGICAL SOCIETY OF EDINBURGH.

### SESSION XLIX.—MEETING VIII.

*Wednesday, 6th April* 1870.—Dr BENNETT, *President*, in the Chair.

I. *Professor Gairdner*, of Glasgow, exhibited a rare and probably unique specimen of TRICUSPID OBSTRUCTION, referred to at page 602 of his work on Clinical Medicine. The patient was a man at that time (1862) aged about 20. He suffered from no ailment or inconvenience, except a peculiar undulatory movement in his neck, originating in the jugular veins on each side of the neck, which were apparently permanently enlarged without being much distended by their contents. The cardiac murmur commenced immediately after the second sound, continued (*diminuendo*) throughout the pause, and then went on (*crescendo*) up to the first sound, when it stopped abruptly. Tricuspid contraction was the diagnosis then made: the result shows it to have been a case of tricuspid obstruction, the exact nature of which it was impossible to predict. From the length of time elapsed not much hope was entertained of ever having the case cleared up. Dr Greig, of Dundee, however, kindly kept his eye on the patient; and hearing recently of his death from some acute disease, for the relief of which he did not apply for medical assistance, he was fortunate enough to secure the specimen. The right ventricle and tricuspid valve were perfectly healthy, the obstruction to the range of the blood into the ventricle being found to be a fibrinous concretion the size of a walnut, which lay in the auricle, and acted to some extent as a ball valve.

II. *Dr Gairdner* also exhibited THE BLADDER AND KIDNEY of a patient he had formerly seen with Dr Page of Carlisle. The urine was acid and otherwise natural, but contained pus, without anything else to indicate whence it had come; in particular, there was no sensitiveness on catheterization. The patient ultimately got worn out, his urine became alkaline, and he died. Numerous small abscesses were found in the pelvis and substance of the kidney, but the bladder was specially diseased, and a probe could be passed under its muscular fibres; it seemed to be an instance of Cruveilhier *ramollissement pultacée* of the bladder.

III. *Dr P. H. Watson* showed a LARGE URIC ACID CALCULUS, weighing 8¼ ounces, and measuring in circumference 9 inches. This calculus was removed by him from a lad æt. 17, by the ordinary lateral operation. The patient survived the operation for four days.

IV. *Dr Watson* also showed THE BLADDER, RECTUM, AND ISCHIO-RECTAL FOSSA from the foregoing case. The mucous membrane of the bladder at one part was villous, and on microscopic examination proved to be affected by epithelial cancer. The interest of the specimen lay in the appearance of the vesical aspect of the incision, which was not more than three-quarters of an inch in extent, proving that, although the prostatic wound admitted of enormous dilatation, it had also the capacity of becoming contracted to its original dimensions, as the incision was not larger than that ordinarily produced by the lateral operation practised on the dead body.

V. *Dr Watson* also showed a SMALL TRIANGULAR URIC ACID CALCULUS removed by him from the urethra of a patient whom he had lately seen in consultation on account of retention of urine. On passing the catheter the existence of the calculus in the perineal portion of the urethra was at once recognised. By means of a pair of dressing forceps the calculus was easily extracted, kneading the calculus forward into the grasp of the opened blades, instead of attempting to grasp the calculus with the forceps, as usually recommended. The introduction of a catheter, after the extraction of the calculus, to evacuate the bladder, revealed the existence of a larger calculus within that viscus, which it was deemed most expedient to leave alone till all irritation following the retention had subsided before resorting to lithotrity.

VI. *Dr Joseph Bell* showed a LARGE ENCEPHALOID TUMOUR, which he had removed, along with the breast, from a lady aged 40. The tumour had been noticed only for nine weeks, and had grown very rapidly. The skin was thinned and purple over the most prominent part of the tumour yet not involved. The tumour was excessively vascular, pretty well encapsuled, and under the microscope its structure was seen to be numerous and very large nucleated cells in a loose fibrous stroma, and to contain much blood.

VII. *Dr Grainger Stewart* introduced a man affected with FLOATING KIDNEY, which had been relieved by a mechanical appliance.

W. D., a joiner, 28 years of age, applied at the Infirmary for advice on 16th December 1869, complaining of a tumour, which occasionally appeared in the abdomen, and unfitted him for work.

*History.*—Patient has been generally healthy—never met with any accident. He is temperate, has a good appetite, excepting when the tumour appears. He was first affected about eight years ago, and for some time it recurred about once a month. It now appears on an average every three weeks, and the discomfort lasts for about eight days. Before it comes down, he feels a sensation of thirst, soreness of eyes, and flatulence. When it is down, his appetite is impaired, and he becomes constipated. He has pain after making water, the water becomes thick, and he has much uneasiness around the tumour. When first seen, a tumour of the shape and size of the kidney was felt on the right side of the umbilicus, with the pelvis directed to the umbilicus, not tender on pressure, and not freely movable. On percussion of the renal regions there was a tympanitic note, the urine was albuminous, contained a good deal of pus and epithelium, but no tube-casts. There was no fever. In a day or two the tumour passed upwards to a position immediately below the gall-bladder, and then disappeared. Coincidently with this, the percussion note over the left renal region ceased to be tympanitic, and became comparatively, but not absolutely, dull. There was no transposition of viscera, nor any other abnormality, excepting a mitral systolic murmur. The albumen disappeared from the urine during the intervals between the attacks. About Christmas, an instrument, prepared by Mr Gardner, was applied so as to press on the point below the gall-bladder, at which the kidney was accustomed to pass forwards. The instrument resembles an ordinary truss. He remained free from any attack till the 30th of March, when the spring of his instrument being broken, the pressure was discontinued. Since it was repaired he has continued quite well.

VIII. *Dr Grainger Stewart* also showed kidney of a man who had suffered from FEBRILE HÆMATURIA, followed by pneumonia and death.

C. M., a shoemaker, æt. 39 years, was admitted on 12th March, complaining of pain in the kidneys and hæmaturia. He had been a soldier, and while serving in India had suffered from ague. Since his return, in 1863, has had several attacks of intermittent fever. Has been an inveterate drunkard, and present illness commenced after a severe drinking bout. He once before had an attack of hæmaturia, without dropsy, from which he recovered in a few days.

On admission, the pulse was 120, soft, and compressible; and continued high and feeble throughout. The temperature was 104° the day after admission. The only other abnormality was the intense pain in both renal regions, and the discharge of urine, of sp. gr. 1025, loaded with blood and blood pigment. There were also numerous hyaline and some bloody tube-casts.

These symptoms continued until the 19th, when he improved considerably; the blood having disappeared from the urine, the pulse slower and firmer, the temperature 100°.

On the 22d he became very much worse, and on the 23d died, without, however, any renal symptoms. He had no cough nor dyspnœa, and, on account of his extreme debility, no physical examination of the back of the chest was made.

On post-mortem examination, pneumonia was found in the lower part of both lungs. The kidneys were enlarged, congested, and in some parts contained extravasations of blood. On microscopic examination, the tubules were found free from exudation. The epithelium for the most part natural. Only in a few of the convoluted tubules the cells were somewhat cloudy; but even these showed the nuclei with great distinctness.

IX. *Dr Grainger Stewart* also showed a specimen of SYPHILITIC TUMOUR IN THE BRAIN. The tumour was the size of an egg, and was situated in the upper surface of the right hemisphere near the vertex; it was in part translucent, and in part cheesy. The history of the case was the following:—The patient had had syphilis at the age of 14, but had enjoyed fair health till January 1868, when she was much alarmed, and was soon after, for the first time, affected with fits; the right side was chiefly convulsed. She lost memory of words for a day afterwards. The fits recurred with tolerable regularity and considerable severity, with occasional improvement under the use of the iodide of potassium, etc., until February 1870, when she died. She became blind during the course of her illness, and on ophthalmoscopic examination, Dr Argyll Robertson reported that there was extensive disease of the right, and to a less extent of the left, optic nerve. The diagnosis of syphilitic tumour of the brain had been made during life.

X. *Dr Lauchlan Aitken's* paper on CHRONIC HEPATIC FISTULA, cured by injection of iodine, was then read.

This paper appears at page 1092 of this Journal.

XI. *Dr Bennett* then read the following paper on THE THERAPEUTICAL EFFECTS OF CHLORAL.

In bringing before this Society a notice of the therapeutical effects of the hydrate of chloral, I am simply actuated by the desire of drawing to a focus the experience of the members regarding it. A discussion on this subject also, by the practitioners of this city, may possibly give an impulse to its administration, and, by indicating the points that merit attention during the empirical trials now everywhere carried on with the new remedy, may ultimately lead to more definite and exact results.

Many of us still remember the enthusiasm with which the great

American discovery of etherization, to remove pain, was received by this country in 1846, as well as what occurred the following year at the meetings of this Society, when chloroform was substituted for ether as a more powerful, and, as it was then thought, equally safe remedy. Unfortunately the occasional sudden deaths which have followed the employment of the last agent, has fostered the trial of other substances. Nitrous oxide gas has again been employed in recent times with great success in the extraction of teeth. The bichloride of methylene, the tetrachloride of carbon, and the chloride and nitrate of amyle, have also been tried with varying results. The hydrate of chloral was introduced only last year by Dr Liebreich of Berlin; and from what I have seen of its action, as well as from the numerous reports made of it in the medical periodicals, it seems to be especially worthy of the consideration of the profession.

In 1830, Liebig discovered chloral by acting on ethylic alcohol with dry chlorine. The thin liquid of the high specific gravity of 1502 so formed, when mixed with water, heats the fluid, which deposits needle-shaped crystals of the hydrate of chloral. These are colourless, readily soluble in water, to which they communicate a mawkish and acrid taste. When further diluted, however, it is easily taken by patients, although the addition of a little flavoured syrup renders it more agreeable.

Numerous experiments have been made upon it on animals by Liebreich, Richardson, Dumarquay, and others: 1 gr. in frogs produces complete insensibility; from 1½ to 2½ grs. in pigeons causes a similar result; in rabbits from 20 to 30 grs. are required for this purpose; and in dogs a larger dose according to their size. The primary effect is upon the cerebral lobes, the second upon the medulla oblongata, and the third upon the heart through the pneumogastric nerve. An excessive dose kills by coma, sometimes accompanied by convulsions, and always with diminution of temperature. I have found the hypodermic injection of 15 grains sufficient to produce profound sleep in a rabbit for two or three hours.

According to Liebreich, chloral is a trichloric aldehyde, which, when dissolved in an alkaline medium, is decomposed and liberates chloroform. This is formed in the blood, from the absorption of the hydrate, and produces its physiological effect more slowly, more permanently, and with less mischief than when introduced by inhalation.

In the pamphlet of Liebreich, and scattered throughout the medical periodical press, are recorded a large number of cases in which doses varying from 5 grs. to ʒi. have been given as a hypnotic. As a comatose agent, destroying sensation so profoundly in man as to permit of painless operations, I am not aware that it has yet been tried. It is not my intention, however, to give even a resumé of all that has been published on the subject of chloral, but rather to state the results of my own trials, and those of some others which have been made in the Royal Infirmary. The former have been tabulated by my resident clinical clerk, Dr Moodie, and show that 64 careful observations as to the effects of chloral have been made on 52 cases in my clinical wards with the following results:—

*1st*, That in only 4 out of the 64 occasions in which 20 grs. of chloral were given, sleep was not produced. In one of these, violent diarrhœa kept the patient awake, and in two others the drug was given early in the morning after a good night's rest.

*2d*, Slight headache occurred on 6 occasions, after sleep produced by the drug; but all these patients, with one exception, had been subject to headache.

*3d*, Vomiting occurred on 3 occasions, twice in one patient who took the solution of chloral immediately after a dose of cod-liver oil.

*4th*, The tongue and appetite were not affected on any of these occasions.

The following observations were made by my son, Dr Alexander Bennett, in the surgical wards, to which he is attached as resident clerk:—

From 4th March 1870 to 4th April 1870, 105 observations have been made upon 42 individuals, divided into two classes:—

1. *Physiological*, when the drug was given to healthy adults or persons with complaints not likely to interfere with the action of the medicine.

2. *Therapeutical*, when the drug was given to relieve suffering, or procure sleep when that was absent.

1. PHYSIOLOGICAL.—20 grains given at 2.30 P.M. 51 observations were made upon 34 healthy adults, with the following results:—

No effect whatever, in 26; felt giddy, or had headache, without sleep, 10; fell asleep within an hour, 15; giddy before sleeping, 5; headache on waking, 7; tongue as before, 51; appetite as before, 51; excited or delirious, none; vomited, none.

25 grains at 2.30 P.M. 23 observations upon 23 healthy adults, with the following results:—

No effect whatever, 10; felt giddy, or had headache, without sleep, 5; fell asleep within an hour, 8; giddy before sleeping, none; headache on waking, 2; tongue as before, 23; appetite as before, 23; excited or delirious, none; vomited, 1.

2. THERAPEUTICAL.—Dose generally 40 grains, given at night. 31 observations upon 20 individuals, chiefly employed for (1.) Sleeplessness from unknown cause; (2.) Sleeplessness from pain or irritation; with the following results:—

No effect whatever, 7; asleep within an hour, steadily all night, 14; asleep within an hour, in snatches all night, 3; asleep within two hours, 5; asleep at any time during the night, 2; headache on waking, 9; excited or delirious, 7; vomited, none; tongue and appetite not changed.

What has struck me with regard to the hypnotic effects produced by chloral, is the fact pointed out by Liebreich, that it causes sleep without exciting the pulse or respiration. In the majority of cases it produces no excitement or uneasiness, nor is it followed by headache, furred tongue, or feeling of depression. With a very few exceptions, indeed, it seems in moderate doses to occasion natural sleep. I have seen rabbits motionless, breathing tranquilly, and evidently fast asleep, who could be roused so as to take a few steps, or eat a little cabbage, and then, overcome by drowsiness, fall asleep again. The same power of being roused from a moderate dose occurs in the human subject. No doubt, the somnolence can be augmented by increasing the dose until it becomes soporose or comatose, when stimulants fail to rouse. Of the intensity of this last effect in man, however, little is known, but, from what has been observed in the lower animals, there can be little doubt that sufficient coma can be produced, so as to admit of operations without pain. Whether, however, it is so easily manageable as ether or chloroform, remains to be seen.

The qualities I have described seem to render chloral highly serviceable to the physician. Although in a few cases it has occasioned excitement before sleep, and headache afterwards, in the majority of cases it is not a narcotic, but a pure hypnotic. The ill effects, so common in the administration of opiates, I have not witnessed on giving chloral at all. For example, the pupil is not contracted more than in ordinary sleep. There is not, for the most part, on waking, the confusion of head, the sense of depression, the furred tongue, want of appetite, nausea or vomiting, suppression of the secretions or constipation, so common after giving opiates. It therefore occurred to me that it would be a most useful hypnotic in phthisis. Here it too frequently happens that cough, pains in the chest, and restlessness at night, if alleviated by opiates, and even by that excellent preparation, chlorodyne, leave the patient so nauseated, depressed, feverish, and weak, as to do more harm than good. Indeed, in my own practice, I have found that such is the loss of appetite, increased weakness, and emaciation, that follow their use, that I never give them, except in the last stages of the disease, and when, all hope of prolonging life having ceased, we choose the least of evils in procuring even unhealthy sleep. But matters are greatly changed when we are able to obtain the natural sleep that chloral produces. By its aid, we can lull irritations, and give rest for a time, in many cases without any injury whatever. I have recently made special observations on this point, in nine cases of phthisis, now under my care in the clinical wards, and have collected the experience of some of my colleagues, with the following results:—

77 observations were made on 21 cases of phthisis, 9 in my own wards, 6 in those of Dr George Balfour, and 6 in those of Dr Grainger Stewart, with the following results:—

In my own cases, only 20 grains were given for a dose; in the others, 30 grains. On every occasion, with two exceptions, deep and healthy sleep was produced. In one case, sleep was delayed for three hours, and in another, after the larger dose, an excited state approaching delirium was occasioned for some hours. In one case it caused vomiting twice, when taken with cod-liver oil, but when dissociated from it, produced no ill effect. In one case, in addition to cough and restlessness at night, there was considerable sweating, which was much alleviated by the chloral. To assure myself of this fact, it was given ten nights running, always producing good effects, and when stopped, the sweating again increased. In one case, slight headache existed in the morning. In one case, also, the tongue was more furred in the morning. In all the other cases, the relief to the usual cough and restlessness at night, with sleep, was most marked, while the head, tongue, and appetite were in no way affected. For the same reasons that chloral is useful in phthisis, it has been found beneficial in certain cerebral diseases in which opium is contra-indicated. In another class of cases, however, the valuable qualities of that drug in checking secretion, while acting as a sedative, will give it superiority. But I refrain from entering into a consideration of the numerous diseases in which the drug has been administered, and which have been recorded by Liebreich and numerous others. The first step, it appears to me, ought to be a determination of its physiological action, and this, so far as experiment and observation have yet gone, points to its being, in moderate doses of 20 to 30 grains, the purest hypnotic we possess. In doses of 30 to 60 grains, it causes excitement, giddiness, delirium, and headache more frequently.

## OBSERVATIONS TO DETERMINE THE ANTAGONISM BETWEEN CHLORAL AND CALABAR BEAN.

In five experiments on as many rabbits, fatal doses of the extract of Calabar bean were injected hypodermically, after the full effect of chloral was produced, with a view of determining how far the poisonous influence of the former was counteracted by the latter. In every case, the convulsions were checked, and life prolonged; but when an unequivocal fatal dose of the Calabar bean was administered, death ultimately resulted.

*Experiment* 6.—One quarter of a grain of Calabar bean extract killed a full-grown healthy rabbit, with violent general convulsions, in 6 minutes.

*Experiment* 7.—Repeated in a similar rabbit; after it had taken 15 grains of chloral, there were no general convulsions, and death occurred in 2 hours and 53 minutes.

*Experiment* 8.—Repeated in a large healthy rabbit weighing

6 lbs. No general convulsions, but insensibility and profuse salivation, paralysis, and general muscular twitchings; died in 1¾ hours.

*Experiment* 9.—Repeated in a similar rabbit, weighing 6 lbs. The same phenomena were produced, and lasted 3½ hours, when the animal slowly recovered.

There can be no doubt that three-quarters of a grain of the Calabar bean extract employed, made by the Messrs Smith of this city, would kill any rabbit in from 6 to 8 minutes. The influence of chloral in counteracting this result may be judged of by the above experiments. It has yet to be determined how far other hypnotics and narcotics may, like atropia, as shown by Dr Fraser, and like chloral as in the above experiments, produce a similar effect.

*Dr Fraser* considered Dr Bennett's contribution a most valuable one. Though not fully worked out, his inquiries into the physiological effects of chloral were most important, inasmuch as they confirmed the observations of numerous previous inquirers in regard to its valuable hypnotic properties. Liebreich had an idea that it acted in some measure like chloroform, but this seemed to be erroneous. No doubt both were hypnotic, but chloroform was something more, it was an anæsthetic. On the other hand, the sleep produced by chloral was characterized, not by anæsthesia, but by retention of reflex excitability, and even by hyperæsthesia. He, and his colleague, Dr Muirhead, had, in a large series of cases, obtained most valuable results from its hypnotic action in fever, and had found that when violent cerebral excitement was present, an ordinary dose was apt to increase it, while a double dose sufficed to allay it, and produce its hypnotic action.

*Dr Gairdner* mentioned one case of chorea in a child of 10, who had been treated with arsenic, belladonna, Calabar bean, zinc, and bromide of potass, with varying and always only temporary success. The case was very obstinate, and was at last put upon chloral in gradually increasing doses, 5, 10, 15, 20 gr. every four or five hours. At last the assistant trebled the dose in the mixture, and the night nurse, not being alive to the change, administered 60 gr. in one dose. The patient became soporose, and seemed in danger, but there was no pallor or coldness of surface; her pupils were much contracted, but relaxed after a time, and when she at length woke up, her chorea was all but completely cured, a slight nervous movement of the hands being all that was left. An old gentleman, whom he had also seen in a state closely allied to delirium tremens, had 15 gr. doses of chloral repeated at short intervals till he got 90 gr. in all; the result was a deep comatose sleep with slight traces of sensibility, but no hyperæsthesia. He woke up next day not well, but improved, and he is now much better. Of several cases he had seen treated by chloral, he had only known of one fatal case, and of this he possessed no details, and was not aware how far the chloral was actually to blame. Dr Russell, of the City of Glasgow Fever Hospital, had obtained very favourable results in typhus from the use of chloral.

*Dr Annandale* had found the use of chloral very satisfactory in surgical practice, and free from the objectionable results of opium.

*Dr David Gordon* had observed a peculiar papular eruption and a form of conjunctivitis as the results of chloral.

*Dr George W. Balfour* had employed chloral very largely during the last three or four months, both in hospital and private practice, with only the most beneficial results; its hypnotic action was readily induced, and produced suspension of various forms of nervous derangement—coughs, pains, and sleeplessness, from whatever cause arising—which was frequently permanent. Hyperæsthesia was a comparatively unfrequent result of its use, it being more frequently attended by various degrees of anæsthesia. The secondary conjunctivitis attending its employment was certainly a remarkable and almost invariable attendant in its prolonged use, for which no local applications seemed to be of any avail; astringent washes seeming to have little effect on it, and the best applications or palliatives seeming to be bathing the eyes with tea, or simple warm water, exposure to the air, and the use of blue spectacles. In two cases he had used the chloral in full doses every night for three months with none but the best results; and, as far as he could observe, without there being any necessity for increasing the dose. The most remarkable results he had obtained with chloral had been in the treatment of delirium tremens and acute mania, in both of which, so far as his experience went, it acted rapidly, safely, and with most remarkable efficiency.

*Dr Thomas G. Balfour* remarked that he had occasionally obtained excellent hypnotic effects from chloral when opium had failed. He understood that Dr Crichton Browne, of the Wakefield Asylum, had occasionally observed the occurrence of purpura in cases of chronic mania. The details of these cases would be published.

*Dr Grainger Stewart* observed, that in one or two cases he had observed distressing symptoms to follow the use of chloral. In one case temporary insanity had occurred, accompanied by delirium, the patient fancying he saw rats and other animals running about him. He had no delirium previously.

*Dr Menzies* stated that he had a case of a child, aged 7, who had no sleep for three weeks from the excessive movements of chorea, in whom three grains thrice a day sufficed to produce quiescence.

*Dr M'Gibbon* observed that in a case of puerperal convulsions chloral had no effect; but in acute mania and delirium tremens he had found it successful, the pulse being lowered, and the surface becoming cool to the feel.

*Dr Bennett* stated that the effect of large doses of chloral seemed to be more narcotic than moderate ones,—that is, they produced nervous excitement, followed by a sedative effect; but in moderate doses of from 20 to 30 grains, we had no remedy of so purely hypnotic a character. He considered all theory as to its action as yet imperfect, yet Liebreich's views seemed to be somewhat confirmed by its beneficial action in typhus, in which disease, according to Dr Russell of Glasgow, it seemed to act strongly, because the blood was undoubtedly ammoniacal. The proper way to judge of the effects of any remedy was to collate a number of cases, record their symptoms, and watch the physiological effects produced. This required union and co-operation among members of the profession, and more especially among hospital physicians and surgeons, an approximation towards which he had shown to exist by the facts furnished to him by his medical brethren, and which had been incorporated in the paper. The period of time this drug had been in use was perhaps as yet too short to afford trustworthy information in regard to all points.

*Dr Fraser* said that he could not approve of Liebreich's views, that chloral, when mixed with blood out of the body, gave forth a chloroform smell; and there was no proof that it formed chloroform within the body. In his experience, a febrile condition frequently diminished the hypnotic action of chloral, and so the argument derived from its use in fever was by no means in support of Liebreich's views. That although solutions of caustic potash and soda no doubt converted chloral into chloroform, *still* these substances were not the causes of the alkalinity of the blood, and in experiments which he had made by treating chloral *with* carbonates, phosphates, etc., at the temperature of the blood, no evidence was obtained of the production of chloroform.

*Dr M'Kendrick* stated, that in the experiments performed in the physiological laboratory of the University, he had observed that in poisonous doses hyperæsthesia was produced before the full action of the drug was manifested; but when this took place, then anæsthesia occurred.

## OBSTETRICAL SOCIETY OF EDINBURGH.

### SESSION XXIX.—MEETING VII.

*23d February* 1870.—Dr Bell, *President*, in the Chair.

Thomas Hardie, M.D., F.R.C.P., was elected an Ordinary Fellow of the Society.

I. *Dr Rattray* showed a LARGE PLACENTA. The confinement had been natural, and the patient's age 36. Her third child weighed 12 lbs.; this one 10¾ lbs. The placenta was expelled in twenty minutes, and the cord measured 29 inches.

### II. PUERPERAL TETANUS. BY W. CRAIG, M.B., ETC.

This paper will appear in a future number of the Journal.

*Dr Keiller* said that Dr Craig's paper was one of great importance, and that he could bear out the remarks made by him on puerperal tetanus. Some years ago he had himself a case of puerperal tetanus during abortion, when the patient recovered. There had been plugging in that case, and some supposed that that was the cause of the spasms. There was the usual painful opening and closing of the jaws. Opium was used as the treatment. Dr K. suggested the use of chloral. The German discoverer of the drug says that chloral presents the convulsive effects of strychnine. Sir James Simpson mentions a case of fatal tetanus following the extraction of a polypus.

*Dr Cochrane* entirely coincided with Dr Keiller's views on puerperal tetanus, but thought that the use of opiates by injection would prove more useful than chloral.

*Dr Cairns* asked whether the patient had vomited, or any appearance of a rash had been detected, to which Dr Craig replied in the negative. Dr Cairns said that he agreed with all that had been stated as to the extreme rarity of this disease. He had consulted numerous works, but could find no notice taken of puerperal tetanus, and he confessed that he had come to the meeting with the intention of disputing whether this was really a case of tetanus at all. But after hearing Dr Keiller's remarks, he was bound to admit that the affection was actually tetanic. As to the suggestion that chloral might have been of benefit in such a case, he thought that it would have been more likely to produce harm. He had tried it on his own patients and on himself, with the effect of producing serious consequences.

*Dr Bell* called attention to the peculiar character of the spasm, and suggested that the woman might possibly have taken strychnine. He also thought that chloral might have been of use.

*Dr Sidey* said he had tried chloral in a case of puerperal convulsions without any benefit.

*Dr Menzies* mentioned that Dr Burn had met with a fatal case of puerperal tetanus after abortion some years ago.

*Dr Ritchie* recollected a case of traumatic tetanus in the Children's Hospital, in which an ounce of Indian hemp had been administered with success.

### III. THE STETHOSCOPE AS A MEANS OF ASCERTAINING THE SEX OF THE CHILD. BY JAMES CUMMING, M.B., C.M.

#### TABLE I., MALES.

The first case was one of twins, the heart of the one fœtus was heard in the right groin beating 110 in the minute, and on delivery it proved to be a male; the second heart was heard in the left hypochondrium beating 154, and on delivery it was found to be a female.

| | | | | |
|---|---|---|---|---|
| 2. | Fœtal | pulsation, | 138 | per minute. |
| 3. | ” | ” | 138 | ” |
| 4. | ” | ” | 135 | ” |
| 5. | ” | ” | 130 | ” |
| 6. | ” | ” | 130 | ” |
| 7. | ” | ” | 132 | ” |
| 8. | ” | ” | 132 | ” |
| 9. | ” | ” | 140 | ” |
| 10. | ” | ” | 132 | ” |
| 11. | ” | ” | 140 | ” |
| 12. | ” | ” | 136 | ” |
| 13. | ” | ” | 133 | ” |
| 14. | ” | ” | 134 | ” |
| 15. | Fœtal | pulsation, | 116 | per minute. |
| 16. | ” | ” | 120 | ” |
| 17. | ” | ” | 120 | ” |
| 18. | ” | ” | 138 | ” |
| 19. | ” | ” | 125 | ” |
| 20. | ” | ” | 140 | ” |
| 21. | ” | ” | 140 | ” |
| 22. | ” | ” | 137 | ” |
| 23. | ” | ” | 140 | ” |
| 24. | ” | ” | 141 | ” |
| 25. | ” | ” | 122 | ” |
| 26. | ” | ” | 120 | ” |

TABLE II., FEMALES.

| | | | | |
|---|---|---|---|---|
| 1. | Fœtal | pulsation, | 150 | per minute. |
| 2. | ” | ” | 142 | ” |
| 3. | ” | ” | 140 | ” |
| 4. | ” | ” | 150 | ” |
| 5. | ” | ” | 144 | ” |
| 6. | ” | ” | 140 | ” |
| 7. | ” | ” | 140 | ” |
| 8. | ” | ” | 144 | ” |
| 9. | Fœtal | pulsation, | 140 | per minute. |
| 10. | ” | ” | 152 | ” |
| 11. | ” | ” | 140 | ” |
| 12. | ” | ” | 143 | ” |
| 13. | ” | ” | 144 | ” |
| 14. | ” | ” | 141 | ” |
| 15. | ” | ” | 160 | ” |

From these two tables it seems that when the pulsation varies from 120 to 140, the probability is that the fœtus *will* be a male, and when the pulsation varies from 140 to 160, the fœtus will likely be found to be a female. But there are some exceptions to these facts. In three cases in which the pulsation was from 150 to 160, the fœtus proved to be a male; and in fifteen cases in which the pulsation varied from 116 to 138, the fœtuses were found to be females. It therefore appears that there is less frequent variation in the pulsation in the male fœtus than in the female; or rather that there are fewer cases in which the heart's action exceeds 140 in the male, than that it falls below that number in the female.

These tables are exceedingly interesting, however, so far as they go; and the subject is well worthy further attention.

*Dr Keiller* remembered that this subject had been investigated a long time ago by Dr John Buchanan, but no reliable results were arrived at.

*Dr Bruce* thought that Dr Cumming's observations tended to prove that we cannot with certainty ascertain the sex, but was of opinion that the subject should be further examined.

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## ODONTO-CHIRURGICAL SOCIETY OF SCOTLAND.

THE annual meeting of this Society was held at 5 St Andrew Square, on Tuesday the 12th May. Owing to the severe indisposition of the president, Francis Thompson, Esq., F.F.P.S. (since dead), the chair was filled on the occasion by Peter Orphoot, Esq., M.D.

After the exhibition of various objects of interest by different members of the Society, Mr D. Hepburn, L.D.S., read a paper upon "Irregularities of the Teeth and their Treatment." The subject was arranged under three heads—viz., its cause, treatment, and the instruments made use of for the reduction of these deformities. Referring to the first of these, its cause, Mr Hepburn took a hasty review of the different theories entertained upon the point; such as selective, and in-and-in breeding, cross-breeding, physical degeneracy, fruitless and finger-sucking, premature extraction of the temporary teeth, etc., remarking that, while there was a certain amount of truth in all of them, to none of them in particular could we look as explaining the sole cause of contracted jaws, or the irregularities in the teeth, to which they give rise; but that we would probably be nearer the truth in ascribing it to a combination of all of these, with other conditions and influences of which we were still ignorant. He then explained the mode of treatment by surgical and mechanical means, describing the various instruments which had been made use of from time to time for this purpose, exhibiting a clasp-frame, which he had found applicable to a large variety of cases, as was evidenced by the number he placed before the meeting, both in a completed and progressive state. The paper was illustrated throughout by a great variety of models and regulating frames. At its conclusion, the chairman explained that it was the practice of the Society to discuss the subject at the meeting following that on which the paper was read, when all the members would have it in a printed form. He would, however, be glad to hear any remarks from the strangers present, or from those members who were not likely to be present at our next meeting. The following gentlemen availed themselves of the opportunity thus granted to speak on the subject:—viz., Dr Dycer; Mr Williamsom, L.D.S., Aberdeen; Mr Campbell, L.D.S., Dundee; Mr Samuel Mossman; Mr D. D. Hepburn, L.D.S., Nottingham. At the conclusion of the meeting, a vote of thanks was passed to Mr Hepburn for his interesting paper.

# Part Fourth.

## MEDICAL NEWS.

Meeting of the General Medical Council.—On Thursday, 28th April, the General Medical Council met in special session for the purpose of considering the Medical Acts Amendment Bill, which has been introduced into the House of Lords by the Lord President of the Privy Council. The first day was occupied by the usual formal proceedings, and by the President pointing out the work before the Council, and the best way of getting through it; also by an explanation, authorized by the Lord President, and by the draughtsman of the bill, of the powers proposed to be conferred by it on the Privy

Council, concerning which it was found that exaggerated notions prevailed in the profession. On the second day, a motion was made by Mr Cæsar Hawkins, and, after some opposition, agreed to, that clause 3 of the Bill, which enacts that, after a certain time, "No person who is not at that date qualified to be registered under the principal Act, shall be registered under that Act unless he has, after examination by a medical examining board appointed under this Act, obtained a license to practise in medicine and surgery under this Act," should be so altered as to substitute the words "certificate of competency" for "license." Next day, however, after a stormy debate, this motion was rescinded, and, on the motion of Dr Storar, the principle of clauses 3 and 13 were accepted, Government considering them fundamental, and declining to consent to any alterations of them. Dr Bennett moved a resolution, which, after a very close division, was carried, that, while maintaining the principles of clauses 3 and 13, provision should be made for securing, as far as possible, "the co-operation of the medical authorities." The object of this resolution is to secure that all who obtain license to register shall be affiliated to one or other of the existing medical authorities, that their proper relation to the profession may be maintained. Dr Parkes then moved a resolution, which was carried, to appoint a deputation to confer with the Privy Council for the purpose of removing ambiguity, and defining the action of the Privy Council. The remainder of that day's session was then chiefly spent in passing the amendments necessary to define and limit the powers of the Privy Council for the instruction of the deputation. On the third day, a motion by Dr Alexander Wood was accepted, to the effect that, in connexion with clause 10, the deputation should be empowered to bring under the Lord President's notice the importance of framing regulations to provide that every person, before entering on his professional studies, be possessed of sufficient general knowledge, and that a register of medical students should be published from time to time, should the Medical Council think it desirable. On the fourth day, a few verbal alterations were made, chiefly substituting the General Medical for the Privy Council as the ostensible moving agent. It was also decided that it was inexpedient for the Council to decide as to varying grades of degrees, but that all medical or surgical titles given by bodies under royal charter should be registered *simpliciter*, on proof being given of possession of the same. And in regard to clause 18, it was left to the option of a student to pass the license examinations either before or after graduation at a university. On the fifth day, the report of the deputation to the Privy Council was received, and the further consideration of the Government Bill proceeded with. Clause 19 was accepted. In regard to clause 20, it was agreed that it is not expedient to confer university degrees on licentiates without additional and higher examination than those required for the mere license. And amendments were recommended limiting the application of paragraph 2 to any medical authority to the time during which such authority should be in receipt of any part of the surplus fund, and subjecting any person taking any of the minor degrees under the clause to the by-laws of the authorities conferring those degrees. In regard to clause 22, it was resolved that the consent of the General Medical Council shall not be deemed necessary to the prosecution of any person contravening the clause, and that it is not expedient that the duty of prosecuting offenders against this clause shall be assigned to the Medical Council or to any Branch Council. On the sixth day, the business of the session was completed; a few unimportant alterations of clause 30 were agreed to. The Bill generally was approved of, on the motion of Dr Parkes; but a much more important motion, by the same gentleman, seconded by Dr Andrew Wood, was rejected by a narrow majority—viz., that it is expedient that a clause should be introduced into the Medical Act, giving the medical authorities power to elect their representatives to the Council on an enlarged basis, power being given to the various medical authorities to settle the mode of election between themselves and their practitioners, who shall be affiliated to them by license or otherwise. The great body of the profession will never be satisfied with anything less than this; and it is extremely satisfactory that, even in so close a body as the General Medical Council, this motion was only lost by a *narrow* majority.

THE CHAIR OF MIDWIFERY IN THE EDINBURGH UNIVERSITY.—We understand that Dr Matthews Duncan, Dr Keiller, Dr Cairns, Dr Alexander R. Simpson, and Dr Sinclair Coghill, are candidates for this vacant chair.

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## OBITUARY.

### SIR JAMES YOUNG SIMPSON, BART.

It is with sincere and heartfelt grief that we have to intimate the death of one whom we are proud to reckon one of our oldest and most valued contributors—Sir James Young Simpson, Bart., Professor of Medicine and Midwifery in the University of Edinburgh. The son of a baker in a country town in Scotland, and the grandson of one who, we have often heard him say, worked as a quarryman in winter and a day-labourer in summer, Sir James raised himself by his own indomitable perseverance to the foremost place in the science which he adorned. Since the days of Boerhaave,—who once received a letter from a Chinese mandarin, addressed "Boerhaave, Physician, Europe,"—no physician has ever been more widely known; and even Boerhaave's name, learned and distinguished though he was, could never have been such a household word throughout the world as that of Simpson; for then we had no railways nor telegraphs, and, above all, we had no chloroform,—neither the means nor the inducement in his days were equal to those in ours. But if no physician was ever more widely known, it is still more true that no man, since the world was, was ever more greatly loved. Humble-minded, kind-hearted, and benevolent to a fault, it would have been strange had it been otherwise. He had his enemies, undoubtedly, as every one so restlessly active, so energetic, and so successful in enforcing his own views must have; but even they could not help regarding him

> "With that stern joy which warriors feel
> In foemen worthy of their steel."

And his very antagonisms were productive of good to mankind by drawing forth the best energies of his opponents.

The greatness of our loss obscures our judgment and prevents us as yet from clearly discerning how great our Simpson was, and how irreparable to mankind is his premature removal; yet, some things we can discern through the mists of our grief. Nero found Rome brick, and left it marble,—Simpson adopted obstetrics when it was the lowest and most ignoble of our medical arts; he has left it a science numbering among its professors many of the most distinguished of our modern physicians. He found the surgical theatre a pandemonium resounding with groans and yells; he has left it a comparative elysium, where, amid happy dreams, the sufferer is painlessly relieved from his woes. He found medicine labouring under a curse of preventible disease, which no one was able or willing to remove; and his writings have done much to prove the possibility of stamping out such diseases, and to encourage the hope that mankind will yet relieve themselves of the incubus of woe and pauperism supinely permitted to exist. As members of a profession placed in the forefront of the battle which science is ever waging with death and disease, and suffering more than all from those zymotic diseases which cut down the very flower of our youth, we owe Sir James a deep debt of gratitude for the energetic manner in which he has advocated the possibility of stamping out these diseases. And even as mere political economists our thanks are due to him; for it is to these diseases that a very large proportion of our pauperism is due.

But there is indeed no department of the medical art which has not received an impulse to progress from the restless genius of Simpson, and space would fail us were we to attempt to mention even a tithe of what he has done, or to recount the many novelties he introduced into obstetrics, medicine, and surgery, whether these were actually new remedies or merely novel applications of old remedial agents. Some of these, as his "air-tractor," and the inhalation of carbonic acid gas, were ultimately found wanting and were dropped; others, as "acupressure," are still *sub judice;* but cerium, chloroform, the stamping-out process of zymotic diseases, and a host of minor but scarcely less important improvements in therapeutics, still remain to attest his energy and success as a reformer in medicine.

In practical obstetrics his skill and enormous experience made him *facile princeps;* in surgery he was a mere ingenious theorist; while in pure physic his success lay more in his *savoir faire* and his unrivalled fertility of resource, in his quickness to perceive the salient points of a case, and in his singular power of adapting his remedies to his patient, rather than in any great or accurate powers of diagnosis: thus, without being defective as a scientific physician, his greatest powers were displayed as a skilful practitioner.

At the meetings of medical and scientific societies, where controversy is life, the coolness of Simpson's temper, his unrivalled memory, and the extent and variety of his information, always enabled him to hold his own, and generally, in appearance at least, and often in fact, to come off the victor; and from his earliest to his latest days there could be no greater intellectual treat than to hear him pour forth, with sweet persuasive eloquence, those masterly impromptu essays with which he was wont to delight the members of the Royal Medical, the Medico-Chirurgical, and the Royal Societies of Edinburgh.

As a writer Simpson has produced no great work; his writings have been mainly a series of masterly but desultory essays, short but exhaustive, and all displaying in an eminent degree his power, singularly his own, of viewing things from all sides, and of supporting the view he wished to enforce by a vast amount of erudite and recondite lore. His extra-professional labours have been mainly antiquarian; and while his writings on these subjects have been most ingenious and speculative, and have displayed a singular amount of curious learning and of diligent and untiring observation and inquiry, and have consequently excited a great amount of interest among the votaries of that science, they have been too purely speculative, and have belonged too much to that *terra incognita* where speculation alone avails, to be of much interest to the uninitiated in this fascinating pursuit.

As a man, however, we all meet him on common ground, and we do not exaggerate when we say that we never knew a more lovable man. His temper was almost imperturbable; in spite of his keen feelings, he had it always in subjection; he never, in all his controversies, forced things to the bitter end,—he was always ready to forgive and forget; and, when confined to what proved to be his deathbed, he exchanged pleasant messages of mutual goodwill and condolence with one of his oldest and most worthy antagonists, who was also laid aside by serious illness. To his enemies he was forbearing, to his friends he was the very impersonation of kindness, ever ready to use his great influence for their advantage, ever willing to advise, and to employ his valuable time in their behalf, whether in furthering their temporal views, in relieving their sufferings, or in soothing their sorrows; and very many of his professional brethren have grateful occasion to recollect his unwearied, devoted, and unrewarded attention to themselves and their families in times of suffering and distress; while none of us can ever forget the pleasant smile and kindly grasp with which he was wont to greet us.

As a Christian, we believe him to have been an humble follower of his Lord. This phase of his character is, however, too sacred to be enlarged on here, and would not have been alluded to but for a mistaken part which his impulsive nature led him for a time to play. *Laborare est orare;* and Simpson was too true a man not soon to discover that the faithful discharge of duty, and the bearing of our cross for Jesus' sake, is a surer proof of our love to Him, and a more certain means of doing good to our fellow-men, than mere preaching and praying in public, however eloquently or impressively these services may be performed. With all his faults, no man ever possessed a larger share of the confidence of his fellow-men; and the grave has never closed over one who will be more widely or sincerely mourned.

Long a martyr to rheumatism, Sir James was about two months ago laid aside from active duty by a severe attack of *angina pectoris*, which recurred at uncertain intervals, and was accompanied by dyspnœa, and latterly by some degree of dropsy. Though great danger was apprehended from the first, the issue was long doubtful, and up to a few days before his death, it was hoped that his valuable life might still be spared for some time, though a restoration to perfect health could not be expected. The end, however, was nearer than supposed; and after a few days of unconsciousness, he quietly breathed his last at ten minutes to eight on the evening of Friday, the 6th of May. At the necropsy, the source of his sufferings and cause of his death was found to be a large, dilated, fatty heart, globular in shape, and weighing eighteen ounces. At the apex of the left ventricle, the wall of which was thinned, an aneurism, about the size of a pigeon's egg, was discovered; all the other organs of the body were fatty. The arteries of the brain were atheromatous in a high degree. The brain itself, that imperial source of all his restless mental activity, was found to be by no means large; it weighed only fifty-four ounces, and was consequently but little above the average of forty-nine and a half ounces. It may be remembered that the brain of Cuvier weighed sixty-four ounces, and that of Abercrombie sixty-three; so that Simpson's brain forms rather an exception to the rule, that mental power depends upon size of brain. On the other hand, it formed a remarkable example of the perhaps more incontrovertible fact, that mental vigour depends upon the number of the convolutions and the quantity of gray matter; for, on being exposed, the brain presented an appearance not soon to be forgotten by those who were privileged to see it, in the apparently increased number of the convolutions and their great size and development.

Though offered a tomb in Westminster Abbey—an honour but charily bestowed on the members of our profession—his friends, we think rightly, preferred to lay him to rest beside his children on the green slopes of Warriston Cemetery; and thither the remains of Simpson were conveyed on the afternoon of the 13th of May, amid all the pomp and ceremonial of a public funeral. The simple forms of Presbyterianism, and the stern reticence of Scotchmen, are not favourable to impressiveness on such an occasion, but no similar event has ever so deeply stirred the heart of Scotland; and the dense crowd collected from all parts of the city and neighbouring country, and ranged along the route of procession—the number and influential character of the public bodies composing that procession, and the number of private carriages which followed in its wake—sufficiently attested the depth and extent of the feeling evoked. Never since the days of Heinrich Frauenlob could it be so truly said, as on that last sad occasion when James Young Simpson, who entered Edinburgh a friendless and unknown baker's son, was conveyed to his last resting-place by the *élite* of his country, that instead of the busy hum and pattering feet of city life—

> " Nur Glockentöne schwirren gar bange durch die Luft
> Nur eine Strasse füllt sich und die führt in die Gruft ;"

lines which the readers of German will pardon us for thus feebly translating—they describe the scene most effectively—

> " And only from the steeple sounds the death-bell's sullen boom ;
> One street alone is crowded—it leads but to the tomb."

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## ROBERT NASMYTH, ESQ., F.R.C.S.E.

It is our melancholy duty to add another name to the medical obituary of last month. Mr Nasmyth, F.R.C.S.E., and Surgeon-Dentist to the Queen, one of our oldest and most respected citizens, died at his house in Charlotte Square, on the evening of Thursday, the 12th May, at the advanced age of seventy-eight years. For some time back Mr Nasmyth has been in but feeble health, although courageously carrying on his professional labours until within the last two months. Indeed, it was not until three weeks ago that his failing strength created much anxiety in the mind of his medical attendants and old friends, Dr Omond and Professor Christison, who was latterly called in consultation.

Mr Nasmyth was born in the year 1792, and educated at the High School of Edinburgh, where he formed the acquaintance of many who became his friends and supporters during his subsequent long and successful career. At the age of fifteen he commenced the study of medicine in the anatomical class of the celebrated Dr Barclay, who very soon discovered the unusual talent of his youthful pupil, more particularly in dexterous and painstaking manipulations. Mr Nasmyth, at the suggestion of his medical brethren, was for some time after this in London, where he acquired an acquaintance with the mechanical department of dentistry, to which branch he had resolved to confine himself.

In 1823, he was elected a Fellow of the Royal College of Surgeons of Edinburgh, Professor Syme being elected the same year. Mr Nasmyth was always of opinion, that to whatever specialty a practitioner might devote himself, he should always be a fully qualified medical man. He was himself, therefore, as might have been expected, not only a skilful dentist, but an accomplished surgeon, and for long was the right-hand man at many of the operations of Wardrop, Liston, Syme, Ferguson, and other eminent men of his day. Besides his appointment of Surgeon-Dentist in the present Royal Household, he held this office to their late Majesties, George IV. and William IV., thus maintaining the same honourable position during the reigns of three successive sovereigns.

As is very generally known, the late Professor Goodsir was for many years an assistant of Mr Nasmyth's; and it is gratifying to record the mutual respect and esteem with which, up to the last, each spoke of the other. Indeed, Mr Goodsir often expressed himself as owing much of his manipulative skill to the years he spent in this capacity. Mr Nasmyth was one of the earliest office-bearers of the Odontological Society of London, having been made Vice-President in 1857. At the first establishment of the Dental Dispensary in Edinburgh, upwards of ten years ago, he cordially entered into and supported the proposal by his junior brother in the profession, Dr Smith, that such a charity should be started, and all along continued one of its medical staff. It is needless to state, that during his lengthened experience he had collected a mass of practical knowledge on every subject in dental surgery, the loss of which will be not a little felt by those of his medical brethren who were wont to consult him in their difficulties. The death of his second son, Dr Robert Nasmyth, who had adopted the profession of his father, left no successor to his practice. Among his sons, another name will not readily be forgotten, viz., that of Major Charles Nasmyth, the hero of Silistria. Altogether, few men have left a reputation more widely known, or will be more affectionately remembered, than he to whose memory we feel this tribute of respect as justly due.

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